THE
AMERICAN NATURALIST

VOL. XLV September, 1911 NO. 53

INHERITANCE OF THE "EYE" IN VIGNA

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CERTAIN races of the cowpea (*Vigna unguiculata*) have the seed coat completely pigmented, others have no pigment, while others have pigment confined to certain areas. In this paper the fully pigmented races are referred to as having solid color, those without pigment as white, while the pigmented area of the partially pigmented seed coats is called the "eye." This eye, when small, is always confined to the region of the hilum, and when large always surrounds the hilum. It varies widely in size and form, as shown in the accompanying illustrations, which show the principal types of eye. In *a* the pigmented area is confined to two patches on opposite sides of the hilum. In *b* the area surrounds the hilum except at its micropylar end.

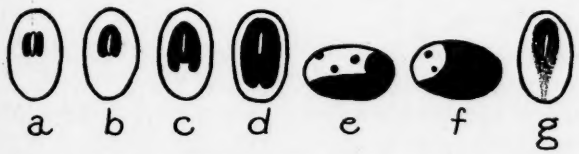

FIG. 1. Forms of the "eye," or pigment area, in the seeds of the cowpea.

In *c* the hilum is completely surrounded, but there is a broad indentation at the lower end of the pigmented area. In *d* the eye covers nearly the entire ventral[1]

[1] The terms ventral and dorsal are used in this paper to describe respectively the side of the seed showing the hilum and the opposite side.

surface of the seed, but has a characteristic notch at the micropylar end (lower end in the figure). In *e* the area has extended over the micropylar end of the seed. In this form and the next there are usually some isolated spots of pigment in the non-pigmented area. In *f* the pigmented area covers all except the dorsal portion of the chalazal end of the seed. Between the forms *a* and *f* there is nearly a complete series of connecting links represented in the material in my possession, but in the present paper I shall not attempt to deal with this whole series, because the genetic relation between some of its members is not yet worked out. Forms *a–c* will here be considered as one, under the name ordinary eye or small eye. Forms *e* and *f* will be treated as one, under the name Holstein, from the color pattern of a variety having this name. The evidence thus far available indicates that form *d*, which is here called large eye, is always heterozygous between Holstein and small eye. Perhaps other of these forms of eye are also heterozygous; material which it is hoped to obtain from the crop of 1911 (F_3) will probably determine this point. There is some evidence that forms *a*, *b* and *c* are the DD, DR and RR of a Mendelian pair. The same is probably true of *e* and *f* and a type intermediate between them. Form *g* of Fig. 1 represents a genetically distinct type of eye. In it the pigmented area surrounds the hilum, but the micropylar end of the area has the margin very indistinct; fine dots of pigment extend over the micropylar end of the seed. In races of this type that are black pigmented the pigment extends over the whole surface of the seed, but it is much denser in the region of the eye than elsewhere. The reduced pigmentation in such cases varies from very dark, rendering close inspection necessary to detect the eye, to very pale, making the eye as conspicuous as it is in buff or red pigmented races. Whether these variations in intensity of pigmentation have genetic significance is not yet determined, but investigations now in progress will, it is hoped, settle this point. This type of eye, in which an ill-defined area of pigment extends over the micropylar

end of the seed (see *g*, Fig. 1), is here called the Watson Eye, from a variety known as Watson's Hybrid, which has this type of eye. This variety, as well as the Holstein variety, is said to have originated from crosses between black varieties and Black Eye. My investigations show that both these color patterns occur in the second generation of all crosses of this kind, as will be seen later, thus confirming the supposed origin of both these varieties.

The Watson type of eye has not heretofore been recognized. But since the essential difference between it and other eye types was noticed, the writer has found in the collection of cowpea varieties in the office of the agrostologist several races having this type of eye. The existence of this type was discovered as a result of the discovery that in several crosses between eyed and completely pigmented races the ratio of fully pigmented to individuals with eyes in F_2 was 3:1 in certain cases and 9:7 in others. The latter ratio suggested that two genetically distinct types of eye were present in certain crosses. A careful study of the rather extensive material at hand revealed the types of eye already described, and a tabulation of the statistical data relating to the inheritance of each of them revealed the relations between these types which are discussed in this paper.

Statistical Data

Cross: Small Eye × *Solid Color* (fully pigmented)

In all, 21 crosses of this kind were made. In all cases F_1 was solid color. It happened that in classifying the individuals of the F_2 generation, before the types of eye present were fully recognized, Large Eye and Small Eye individuals were not separated in 5 of the 21 families. In the remaining 16 families, taken together, the proportion of the various types of color pattern in F_2 was as follows:

	Solid Color	Watson Eye	Holstein	Large Eye	Small Eye
Number of individuals in the 16 families taken together	127	41	12	34	12
Highest expectation	126	42	14	28	14

on the assumption that the ratio between these various types is 9:3:1:2:1. The reason for assuming this ratio will be given later.

In the 5 families in which the large and small eyes were not separated, and in which the corresponding expectation is the ratio 9:3:3:1, the results were:

	Solid Color	Watson Eye	Holstein	Large and Small Eye
Number of individuals in 5 families taken together	100	30	34	16
Highest expectation	101	34	34	11

In both these groups the actual numbers are in close accord with the theoretical numbers called for by the assumed ratios.

Cross: Holstein × Solid Color

Six crosses were made between various types of the Holstein pattern and solid-colored races. The F_1 plants all produced seeds that were fully pigmented. In F_2, taking the six families together, there were 75 individuals bearing fully pigmented seeds and 25 with Holstein seeds, or exactly the ratio 3:1.

Cross: Watson Eye × Solid Color

Two of the 4 Watson parents used in these matings proved to be heterozygous. The other two, taken together, gave 56 solid color and 13 Watson individuals in F_2. F_1 in all these cases was fully pigmented. The 3:1 ratio for the number of individuals in F_2 is 52:17, which agrees fairly well with the experimental results.

Cross: Small Eye × Holstein

Only one cross of this character was made. It gave F_1 with large eye; in F_2 there were 3 Holstein, 4 Large Eye and 1 Small Eye individuals, which is near the ratio 1:2:1 for these three types of color pattern.

MENDELIAN FACTORS INVOLVED

We may bring together here the various ratios found in the above classes of matings. For convenience let us designate the various color patterns as follows:

S = Solid color (fully pigmented seed coat).
W = Watson Eye.
E = Small Eye.
LE = Large Eye.
H = Holstein.

Crosses	Ratios
E × S	9:3:1:2:1 or 9:3:3:1.
H × S	3:1.
W × S	3:1.
E × H	1:2:1.

These ratios point clearly to the following conclusions:

1. Types E and S differ from each other in two factors each of which exhibits the phenomenon of dominance or partial dominance.

2. Types H and S differ in one factor which shows dominance.

3. Types W and S differ in one factor which shows dominance.

4. Types E and H differ in one factor, the heterozygote being intermediate between the parental types.

It is not difficult to formulate, in terms of the customary hypothetical factors, an hypothesis that readily explains these facts. In fact, I have been able to formulate four such hypotheses. In each of these a certain set of factors is assumed, each factor being assumed to have the power of producing a particular effect on the color pattern of the seed coat. But such explanations offer no suggestion as to the real nature and *modus operandi* of these factors. Unfortunately we do not know much concerning the complex chemical processes that go forward in the living cell. But enough is known to show that differences such as are exhibited by the various color patterns here considered may be due to some such cause as slight difference in the rate at which some enzyme may be produced in the cells of the seed coat. For instance, it is known that an enzyme may cause certain sugars and chromogens to unite to form a glucoside, thus removing the chromogen from the sphere of action of an oxidizing enzyme that might con-

vert it into pigment.[2] Another enzyme, or, under appropriate conditions, the same enzyme that brought about the synthesis of sugar and chromogen into glucoside, may hydrolyze glucoside into sugar and chromogen. It is also known that the rate at which a chemical reaction determined by an enzyme goes forward depends on the amount of enzyme present. It is therefore readily seen that the rate at which a given enzyme is produced in the cell may determine whether or not chromogen shall be available for conversion into pigment. It is also highly probable that a principal function of the chromatin of the cell is to produce the enzymes which govern at least the rate of many of the metabolic processes in the cell.[3]

It is certain that environmental conditions during ontogeny determine whether pigment shall be produced in a given tissue, even when the potentiality of pigment production is known to be present. Thus, in seeds that have pigmented coats, pigment may not occur, say in the endosperm. Causes similar to those which determine the particular tissue to be pigmented may also determine what portion of that tissue shall be pigmented. The sugar in the pigment cells of the seed coat is presumably transported there by osmosis from cells some distance away. Other materials necessary to the reactions may be brought from other parts of the organism, and some of the products of a reaction the accumulation of which might retard the reaction may be transported to other parts of the organism as they are produced. We thus have to deal with an exceedingly complex problem, many of the elements of which can not even be conjectured in our present ignorance of cell metabolism. It would therefore be idle to attempt to formulate a definite theory of the processes involved. A slight change in the permeability of certain cell walls, a change in the size of certain cells, the substitution of

[2] "On the Formation of Anthrocyanin," M. Wheldale, *Jour. of Gen.*, I, No. 2.

[3] Guyer, M. F., "Nucleus and Cytoplasm in Heredity," AMER. NAT., May, 1911.

a slightly different group of atoms for a single side chain in a molecule of chromatin or one of the constituents of chromatin, or any one of numerous other changes, might determine whether pigment is to be formed in a given cell, or whether it may be produced at all. But if the change which is responsible for the difference between two related organisms is a change in any permanent organ of the cell, then the difference in question will be hereditary. If it occurs in the material of a chromosome, or any other cell organ that behaves as a chromosome does in the reduction division, the difference in question will Mendelize.

Let us suppose, merely for purposes of illustration, that the difference between fully pigmented seed coat and the Holstein coat pattern is due to a difference in the rate at which a particular chromosome manufactures a particular enzyme under given conditions. Then when these two patterns are crossed we should get the usual phenomena of monohybridism, with the ratio 3:1 (or 1:2:1). Thus the Holstein pattern is not necessarily due to the "loss" of a "factor"; it may be due to some such cause as a difference in the quantity of an enzyme produced by a particular chromosome. The use of such expressions as "presence of a factor" and "absence of a factor" in what follows is therefore not meant to imply the presence of a morphological entity in one race and its absence in another. It rather means that in one race some cell organ, probably a chromosome in Mendelian inheritance, performs a certain function differently, or under different conditions, in the two races. Since the phenomena of Mendelian inheritance point clearly to the physical behavior of some cell organ, I prefer to think of the symbols used in expressing the genetic constitution of a type as representing the bodies, differences in the functions of which give rise to the character "pair."

Thus the symbol W in what follows may be considered as representing a cell organ which, under certain conditions, performs a certain function in such a way as to account for the difference between Small Eye and Wat-

son Eye, while w represents the same cell organ, or rather the corresponding organ, in another variety, which does not perform this same function in the same way under similar conditions. The symbol w need not imply that the power of performing any function is lost. It is here meant to imply only that the function is not performed in such manner as to produce the effect that W would have produced had it been present. Another way of putting it is that W represents a function performed, while w represents that the function is not performed. With this understanding of what is meant by a Mendelian "factor," we may proceed to examine the hypotheses which explain the statistical results given above.

The behavior of the cross: small eye × solid color indicated that these two types differ in two factors which are transmitted independently of each other. Let us represent these factors as they appear in fully pigmented peas by W and H, and in small-eyed peas as w and h. This implies, according to my conception of Mendelian factors, that some cell organ (W), probably a chromosome, performs a certain function in certain races of peas that is either not performed, or is performed differently, by the corresponding organ (w) in another race of peas. A similar remark applies to H and h. With reference to these two factors, the formula of fully pigmented peas may be written WWHH, and of small-eyed peas wwhh. We have already seen that the Holstein type differs from the type with small eye in one factor; also from fully pigmented in one factor. Hence it must have either the formula WWhh or wwHH. That is, in one of the two factors concerned it is like Solid Color, in the other, like Small Eye. We may therefore take wwHH as the formula of the Holstein type. In an exactly similar manner we arrive at the formula WWhh for the Watson type. We may bring these formulæ together for purposes of comparison.

(1) Solid Color : WWHH.
(2) Watson Eye : WWhh.
(3) Holstein : wwHH.
(4) Small Eye : wwhh.

These formulæ give the ratios previously assumed in dealing with our statistical data for the F_2 generation, provided we assume W and H dominant respectively to w and h.[4] An interesting deduction from them is that the cross between the Watson and the Holstein types should give the same ratio in F_2, namely, 9:3:1:2:1, that the cross between solid color and small eye gave. In the original crosses, the cross Holstein×Watson was not included, but it is included in crossings now being made.

These formulæ give some hint as to the nature of the effect produced by the factors W and H. Comparing (4) and (3), the factor H has the effect of enlarging the pigmented area from the small eye type to the Holstein type (see *b* and *e*, Fig. 1). Comparing (2) and (4), W is seen to have the effect of enlarging the pigmented area of the Small Eye type, changing it to the Watson type (see *g*, Fig. 1). W and H together, even in the hybrid WwHh, have the effect of spreading pigment over the whole seed-coat.

We have seen that the heterozygote between Holstein and Small Eye, which has the formula wwHh, is intermediate between the parent forms. In earlier pages this type has been designated Large Eye. It therefore appears that H duplex enlarges the pigment area about twice as much as H simplex does. Whether the same is true of the factor W has not yet been determined. As the ratio 9:3:1:2:1 is somewhat unusual, it may not be out of place to illustrate the manner of its occurrence. In the cross: Small Eye×Solid Color, F_1 has the formula WwHh, and is solid color. Generation F_2 is as follows:

F_2 OF THE CROSS SMALL EYE × SOLID COLOR. = wwhh × WWHH						
		Solid Color	Watson	Holstein	Large Eye	Small Eye
1	WWHH	1	—	—	—	—
2	WWHh	2	—	—	—	—
1	WWhh	—	1	—	—	—
2	WwHH	2	—	—	—	—
4	WwHh	4	—	—	—	—
2	Wwhh	—	2	—	—	—
1	wwHH	—	—	1	—	—
2	wwHh	—	—	—	2	—
1	wwhh	—	—	—	—	1
		9	3	1	2	1

[4] The dominance is only partial in the case of H and h.

Should it later prove possible to separate the two Watson F_2 types, WWhh and Wwhh, as we can the corresponding Holstein types, we should then have the interesting ratio 9:1:2:1:2:1.

There is evidence in my material that the Holstein and Small Eye types are still further influenced by one or more additional factors. This matter is now under investigation. These additional factors appear to act in a manner entirely similar to the factor H, but have less effect. Taking the formula for Small Eye and adding a factor I, similar in effect to H, we should have the three types wwhhII, wwhhIi, and wwhhii, all three of which appear to be distinguishable, giving probably the three types *a*, *b* and *c* of Fig. 1. This point will be studied in F_3 of the original crosses, of which a vast quantity of material is growing, as well as in new crosses now being made.

OTHER HYPOTHESES

In the above hypothesis the factors W and H were assumed to have the property of enlarging the pigmented area. The facts can also be explained by assuming that w and h have the power of reducing the pigmented area. In fully pigmented races, on this hypothesis, these factors are absent. When w is introduced the pigmented area is reduced to the Holstein pattern. h, without w, reduces it to the Watson pattern, while w and h together produce the Small Eye pattern. It is necessary in this case to assume W and H dominant, respectively, to w and h, as in the first hypothesis.

We may also assume a factor W for Watson Eye, a factor, E, allellomorphic to W, for Small Eye, and a third factor, S, which tends to enlarge the pigmented area, converting Small Eye into Holstein, and Watson Eye into Solid Color. Here, W must be epistatic to E. Under these assumptions the formulæ of the principal color patterns would be:

Solid Color	:	WWSS.
Holstein	:	EESS.
Watson	:	WWss.
Small Eye	:	EEss.

A fourth hypothesis is as follows:

H = a factor for Holstein pattern.
E = a factor for Small Eye pattern.
S = a factor which enlarges the pigmented area, converting E into Watson, and H into Solid Color.
H is here epistatic to E.

All four of these hypotheses are in complete agreement with the statistical data, and lead to exactly the same types, and the same ratios, in all generations.

If we look upon the symbols as representing cell organs, differences in whose functions are responsible for the phenomena observed, then these four hypotheses are identical. They all provide exactly the same set of cell organs. The differences between the hypotheses are found only in the nature of the functions which these cell organs are supposed to perform. As we know nothing definite about these functions, the hypotheses are essentially identical within the range of present knowledge. The functions assumed for the bodies represented by the symbols of the first hypothesis seem to the writer to accord more nearly with our meager knowledge of cell chemistry.

The author desires to acknowledge his indebtedness to Mr. G. W. Oliver, who not only performed the cross-fertilizations planned by the writer, but freely made available similar extensive material of his own, which is included with my own in these studies. Mr. Oliver also grew the F_1 hybrids. He is also under obligations to Mr. J. W. Froley, Mr. E. D. Carmack and Mr. W. R. Humphries, who grew the F_2 plants and made the necessary field notes; to Mr. E. P. Humbert, who rendered valuable assistance in classifying the F_2 material, and to Professor C. V. Piper, agrostologist of this bureau, who furnished seeds of the varieties used as parents in these investigations, and made many helpful suggestions concerning probable "factors" present in the varieties.

HEREDITY OF HAIR FORM AMONG THE FILIPINOS

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WHILE connected with the Philippine Medical School in Manila, in the year 1909 two of my pupils, Maria P. Mendoza and Manuel Ramirez, became interested in the heredity of hair form through the work of Gertrude C. Davenport and Charles B. Davenport[1] and they collected the records of 36 families, largely Chinese-Tagalog crosses, although two families were Negritos who had married Filipinos.

They tested the hair form by making sections of hair dipped in thick celloidin hardened in 70 per cent. alcohol and cross-sectioned with a hand microtome, after which the sections were examined under the low power of the microscope, the measurements being made with an ocular micrometer. They divided the forms of hair into—

Straight, with diameters of 100: 90 or over,
Wavy, with diameters of 100: 70–90,
Curly, with diameters of 100: 60–70.

They decided that wherever a union occurred between individuals with straight and wavy hair the straight hair predominated. It seems expedient, however, to consider the hair form in single families as well as *en masse*.

In any discussion of heredity it seems necessary to consider individuals rather than the mass, except in the formulation of laws that take into consideration probable errors in the mass. In Table I, showing the result, in mass, of crossing different hair forms, we should expect, if Mendel's laws prevail, where wavy and straight are crossed or where curly and straight are crossed, to get an equal number of offspring with straight and curved hair

[1] AMERICAN NATURALIST, Vol. XLII, April, 1908.

TABLE I

COMPILED BY MENDOZA AND RAMIREZ

No.	2d Parental FF FM	2d Parental MF MM	Parent F M	Children C	Children W	Children S	Nature of Mating		Expectation C	Expectation S
1	W S	S S	W S	3	1	1	DR	R	2.5	2.5
2	=	=	W S		3	4	DR	R	3.5	3.5
3	=	=	S S	3		3	R	R		6
4	=	W S	S W	1	5	2	R	DR	4	4
5	=	— W	S W		1	1	R	DR	1	1
6	S S	S S	S S	1		2	R	R		3
7	=	C S	S C	1	0	2	R	DR	1.5	1.5
8	C C	W W	C W	6	1	0	DR	DR	5.3	1.7
9	S —	— S	S W	1		1	R	DR	1	1
10	C S	S S	W S	4	1	2	DR	R	3.5	3.5
11	C S	S S	S S		1	1	R(?)	R		2(?)
12	C —	— C	C W	2		1	DR	DR	2 3	.7
13	—	S W	W S	2	2	1	DR	R	2.5	2.5
14	C W	S S	C S	2	1	1	DR(?)	R	2	2
15	C W	S S	C S	1	1	1	DR	R	1.5	1.5
16	C W	S S	C S	2	1	1	DR	R	2	2
17	= S	K K	S(C) K	4			DR	DR	4	0
18	S(C) S	S S	S C			3	R	DR	1.5	1.5
19	S S	S S	S S			8	R	R		8
20	S S	S S	S S			4	R	R		4
21	S S	S S	S S			6	R	R		6
22	— W	=	W W	2	4	1	DR	DR	5.3	1.7
23	S S	C C	S C	1	1	1	R	DR	1.5	1.5
24	S S	=	S W	1		3	R	DR	2	2
25	S S	=	S W			5	R	DR	2.5	2.5
26	S S	S —	S C	1		5	R	DR	2.5	2 5
27	S S	C S	S C			3	R	DR	1.5	1.5

No.	2d	Parental	Parent	Children			Nature of Mating		Expectation	
	FF FM	MF MM	F M	C	W	S			C	S
28	S C	S S	S C	1		2	R	DR	1.5	1.5
29	C C	S S	C S	2		1	DR	R	1.5	1.5
30	S S	=	S C	2		3	R	DR	2.5	2.5
31	S S	S S	S W	1	1	4	R	DR	3	3
32	K K	S S	K S		4		DD	RR	4	
33	=	=	C S	2	1	1	DR	R	2	2
34	=	=	W S	1	1	5	DR	R	3.5	3.5
35	=	=	W S	1	3	2	DR	R	3	3
36	=	S S	S W	1	2	3	R	DR	3	3
37	—y S	— WC	W C	3		2	DR	DR	3	1.25
38	S S	— W	S W		3	7	R	DR	3.5	3.5
39	=	S C	S C	3			R	DR	1.5	1.5
40	=	=	S C	1	1	1	R	DR	1	1
41	— 3	S C	W S	1	5	6	DR	R	6	6
42	=	=	C C	2			DR	DR	1.5	.5
43	— S	C —	W W	1	1	2	DR	DR	3	1
44	W C	S W	W W	2	1	2	DR	DR	3.7	1.25
45	=	C —	S C	2		8	R	DR	5	5
46	=	C S	S W	3		4	R	DR	3.5	3.5

F = father, M = mother, D = dominant, R = recessive, S = straight, W = wavy, C = curly, K = kinky.

where the curved variety is a dominant heterozygote and the straight is a recessive homozygote. Such is the case, for of the 31 families examined in which wavy is crossed with straight or curly with straight, there are 157 children of whom 84 have straight hair and 73 have hair curly or wavy. Where the wavy and the curly are crossed, if each is a heterozygote with the straight recessive, we should expect to find three children with the curved

proportion, for of the five families where wavy hair is crossed with curly, there are 33 children with hair of the curved variety and eight with straight hair.

When individual families are considered, however, there seems to be an alteration of the dominance in some cases. Straight hair is dominant in some families, whereas in others the wavy or curly is dominant. In Table I straight hair appears to be dominant in the families numbered 3, 6, 11, 18, 24, 25, 26, 27, 31, 34, 38 and 45. In the first three of these families where both parents have straight hair, and particularly in family 6, curly hair is evidently recessive. If the gametic composition of the two parents in family 3 is Dr×Dr, in which D = straight hair dominant and r = curly hair recessive, then the expected proportion of offspring would be 3 straight to 1 curly haired child. The number of curly-haired children is 3 instead of 1, which is greater than it should be. But if the straight hair be recessive in this family, the gametic composition must be rr×rr where r is straight recessive, and all of the children should have straight hair, which is not true. In any case the number of curly-haired children is in excess of what it should be.

The curved form of hair seems to be dominant in a greater number of families than is the straight, as in families numbered 1, 4, 10, 12 to 16, 22, 28, 29, 33, 35, 36, 39, 41, 43 and 44. In family 1 the proportion of curly-haired children is in excess (excessive dominance) of what it should be even though the curved be dominant. The same is also true for families 4, 10, 13 and 39. There is, on the other hand, imperfect dominance (recessive dominance) of the curved hair in families 14, 15, 16 and 29; or, in other words, there are fewer children with curved hair in these families than would be anticipated if the curved form be dominant.

When kinky hair and straight hair are crossed, as in families 17 and 32, there results an intermediate form—in one family all the children have curly hair, in the other all have wavy hair. The result of crossing two of these

intermediate forms is not known unless such a cross is represented by families 8, 12, 22, 37, 42, 43 and 44. There is a tendency toward segregation in these families, but no kinky hair appears.

Judging from this cursory analysis of the families under consideration, the inferences are that in the crossing of different grades of hair-form there is a tendency towards segregation or alternate heredity, a modified Mendelism, for there is not a perfect dominance, nor the opposite, for either of the extreme grades of hair form, kinky (woolly) or straight.

It would be of interest to know what relationship the curly, wavy or kinky hair of the European bears to the same sort of hair of the Negrito. Presumably all of the curly or wavy hair in the families under consideration is of European origin except in families 17 and 32 of Negritos, but in no instance can the previous inclusion of Negrito blood be absolutely excluded. Some of the wavy or curly hair may therefore be due to previous crosses of the Filipino and Negrito. Presumably, also, all of the straight hair is derived from the Filipino, but again the European or Chinese straight hair can not be excluded. It may be that the straight hair of the Chinese and of the European would each have a different reaction in heredity with the curved variety of both the European and the Negrito. Unfortunately, observations to determine this may not be possible owing to the mixed condition of the population at the present time.

It may be well to tabulate the families in which the parents are alike and unlike in hair form and to note the kind of hair of the children, in order to determine somewhat exactly the relative behavior of the different forms of hair in heredity. The following crosses will be considered: Group I, straight×straight; group II, curly× curly; group III, wavy×wavy; group IV, straight× curly; group V, straight×wavy; group VI, curly×wavy.

In this and subsequent tables F. = father, M. = mother. C. = curly. W. = wavy, S. = straight, F.F. =

TABLE II

GROUP I, STRAIGHT × STRAIGHT

No.	F.F.	F.M.	M.F.	M.M.	Children			Parents	
					C.	W.	S.	F.	M.
3	—	—	—	—	3		3	S	S
6	S	S	S	S	1		2	S	S
11	C	S	S	S		1	1	S	S
19	S	S	S	S			8	S	S
20	S	S	S	S			4	S	S
21	S	S	S	S			6	S	S

father's father, F.M. = father's mother, M.F. = mother's father and M.M. = mother's mother.

Here are three families (3, 6, 11) in which the parents appear to be simplex or heterozygotes, and three (19, 20, 21) in which they appear to be duplex or homozygotes, and the straight hair is dominant to the curved variety. The parents in the first three families would have a gametic composition of Dr×Dr in which D represents the dominant character, straight hair, and r represents the recessive, curved hair. In any cross of this nature an equal number of children with straight hair and with the curved variety would be expected and the expectation is realized. The parents in the second three families would have a gametic composition of DD×DD, where D represents the dominant character, straight hair, and only straight-haired children should result from such unions. The gametic composition of the last three families could be rr×rr, where r is recessive straight hair, but if straight hair acts as the dominant character in the first three families there is no reason to suppose that it would be recessive in the last three.

TABLE III

GROUPS II AND III, CURLY × CURLY AND WAVY × WAVY

No.	F.F.	F.M	M.F.	M.M.	Children			Parents	
					C.	W.	S.	F.	M.
42	—	—	—	—	2			C	C
22	—	W	—	—	2	4	1	W	W
43	—	S	C	—	1	1	2	W	W
44	W	C	S	W	2	1	2	W	W
Total					7	6	5		

Family 42 is apparently duplex or homozygote with a gametic composition of cc×cc, but there is nothing to indicate whether the curly hair is dominant or recessive. The wavy haired families behave as if simplex or heterozygote, producing both straight and curly-haired children. If wavy hair is produced by a combination of straight and curly it should behave as it does, representing the heterozygote of the F^1 generation, and in the next generation (F^2) there is a return of the two original forms of hair, curly and straight, and a continuance of the wavy.

TABLE IV

GROUP IV, STRAIGHT × CURLY

No.	F.F.	F.M.	M.F.	M.M.	Children			Parents		Children		
					C.	W.	S.	F.	M.	C.	W.	S.
7	—	—	C	S	1		2	S	C			
14	C	W	S	S				C	S	2	1	1
15	C	W	S	S				C	S	1	1	1
16	C	W	S	S				C	S	2	1	1
18	S (C)	S	S	S			3	S	C			
23	S	S	C	C				S	C	1	1	1
26	S	S	S	—	1		5	S	C			
27	S	S	C	S			3	S	C			
28	S	C	S	S	1		2	S	C			
29	C	C	S	S				C	S	2		1
30	S	S	—	—	2		3	S	C			
33	—	—	—	—				C	S	2	1	1
39	—	—	S	C				S	C	3		
40	—	—	—	—				S	C	1	1	1
45	—	—	C	—	2		8	S	C			
Total					7		26			14	6	7

In some straight×curly families, curly hair seems to be dominant, as in 14, 16, 29, 33 and 39. Whereas in others straight hair seems to be dominant, as in 7, 18, 26, 27, 28, 30 and 45. It will be noted that whatever the character of the father's hair, that character is dominant except in family 39. It is also to be noted that straight hair is dominant when three of the grandparents have straight hair, whereas curly hair is dominant when only two of the grandparents have curved hair. If the hair form of the dominant parent is duplex no recessives are to be expected, but if the dominant parent is simplex half

of the children will have little or no dominant hair form. Neither of these expectations is met, except in the families where curly hair appears dominant and there the latter hypothesis fits the facts—14 children have curly hair, 13 have not. We may be dealing with a case of dihybridism in which there is one masked character.[2] The two pairs of characters may be: straight dominant to its absence (curly), and straight dominant to wavy. We should expect on this supposition to get straight, wavy and curly in the proportions: straight 9, wavy 3 and curly 4. The actual number of children with the different kinds of hair is straight 33, wavy 6 and curly 21.

The results of crossing straight and wavy hair may profitably be considered at this point.

TABLE V

GROUP V, STRAIGHT × CURLY

No.	F.F.	F.M.	M.F.	M.M.	Children			Parents		Children		
					C.	W.	S.	F.	M.	C.	W.	S.
1	W	S	S	S				W	S	3	1	1
2	—	—	—	—		3	4	W	S			
4	—	—	W	S				S	W	1	5	2
5	—	—	—	W		1	1	S	W			
9	S	—	—	S	1		1	S	W			
10	C	S	S	S				W	S	4	1	2
13	—	—	S	W				W	S	2	2	1
24	S	S	—	—	1		3	S	W			
25	S	S	—	—			5	S	W			
31	S	S	S	S	1	1	4	S	W			
34	—	—	—	—	1	1	5	W	S			
35	—	—	—	—				W	S	1	3	2
36	—	—	S	S	1	2	3	S	W			
38	S	S	—	W		3	7	S	W			
41	—	S	S	C				W	S	1	5	6
46	—	—	C	S	3		4	S	W			
Total					8	11	37			12	17	14

The father is again prepotent, but there are more exceptions in the straight×wavy cross than in the straight ×curly cross. A great many more curly-haired children (20) appear in the former than wavy-haired children (6) in the latter. The additional factor of dominance associated with the male parent should be noted.

[2] "Mendelism," by R. C. Punnett, Cambridge, 1907, pp. 44–52.

Davenport and Davenport[3] have apparently overlooked the fact that the mother rather than the father seems to favor dominance in the American families in which the hair form is recorded by them. When the mother has straight hair in a cross of curly and straight, the number of children is 4 curly to 24 straight, but when the mother has curly hair in a similar cross, the number of children is 10 curly to 22 straight. It may be significant that in America, where curly hair is dominant and straight recessive, the female parent is associated with dominance, whereas in the Philippines, where straight hair is dominant and curly recessive, the male parent is associated with the dominant hair form.

It is to be regretted that no observations as to the sex of the children were made by either Davenport and Davenport or Mendoza and Ramirez, because if that had been done the repulsion of maleness or femaleness to one form of hair or another might have been determined, in view of similar conditions that exist in the currant moth and in canaries.[4] It appears, however, that femaleness exerts a repelling influence upon the dominant character among Filipinos but maleness exerts the same influence among Americans.

Returning to the cross of straight and wavy hair, it is to be noted that if straight hair is duplex and wavy hair simplex, then the supposition is that half the offspring shall have straight hair and half wavy hair, and there shall be none with curly hair. But if straight and wavy hair represent two different stages of hair-form either of which may be epistatic to curly, then a certain proportion (usually more than 25 per cent.) of the offspring should have curly hair. Actually there are many children with curly hair (a little over 25 per cent. of all offspring) and consequently the second supposition is favored.[5] We

[3] *Op. cit.*

[4] "Mendelism and Sex," by C. C. Hurst, pp. 1–34, an address delivered to the Mendel Society, March 29, 1909.

[5] Cf. Davenport and Davenport, "Skin Pigmentation in Man," THE AMERICAN NATURALIST, Vol. XLIV, No. 527, November, 1910.

may suppose that the gametic composition of the wavy hair is that of the intermediate form between curly and straight, a heterozygote, cs, and that half of the straight hair is simplex, Sc, and half is duplex, SS. The result of mating curly and straight hair would be offspring in the proportion and with the gametic composition of 2SS-straight hair, 1cc-curly hair, Ics-wavy hair, and this proportion is approximately the truth. There are in Table V 51 children with straight hair, 20 curly-haired, and 28 wavy-haired children.

Only three families occur with a cross of curly and wavy hair. Therefore no conclusions may be safely drawn from them. The curly hair does appear to be dominant in these families, and the straight hair recessive, however, because there are 11 curly-haired children, 3 straight-haired, and 1 child with wavy hair.

The two Negrito families where kinky, or woolly, hair is crossed with straight, have four children with curly hair in the family where one grandmother probably had curly hair, and four children with wavy hair where no such presumption exists. There can be no doubt that in a cross of straight and woolly hair the hair-form blends in the first generation. Curly and wavy hair appears wherever the Negrito and the Malay have been in contact for any length of time in the Philippine Islands, and especially among those who are called Negritos, as the Bataks of Palawan, the Negritos in Mindoro, Panay, Zambales, Cagayan and among the Ilongots. Inferences from the present study are that straight hair predominates in families where the ancestry has a predominance of straight hair. Therefore, the inverse of this should be true, namely, that woolly hair should predominate where the ancestry shows a predominance of woolly hair. This question might be decided by records from Negrito families where both parents have wavy or curly hair. The dominance of straight hair over curly has been indicated in previous studies by the author as in the proportion of

84 straight-haired to 24 curly-haired Filipinos of the Primitive type.[6]

To compare the results obtained from the previous records they may be assembled in one table.

TABLE VI

THE DISTRIBUTION OF THE HAIR AMONG THE CHILDREN OF DIFFERENT MATINGS

Mating	Occurrence				Relative Proportion		
	Total	Curly	Wavy	Straight	Curly	Wavy	Straight
Curly × curly	2	2			100		
Curly × wavy	15	11	1	3	73.3	6.6	26.6
Curly × straight	60	21	6	33	35.0	10.0	55.0
Wavy × wavy	16	5	6	5	31.2	37.6	31.2
Wavy × straight	99	20	28	51	20.2	28.3	51.5
Straight × straight	29	4	1	24	13.8	34.5	82.7
Kinky × straight	8	4	4		50.0	50.0	
Total	229	67	46	116	29.2	20.1	50.7

Three classes may be segregated, taking the relative proportions of curly hair alone. In the first class neither parent shows the dominant character and 100 per cent. of the children have curly hair, which would be anticipated from Mendelian crosses of R × R. In the second class only one parent shows the dominant character and the curly-haired children constitute approximately 50 per cent.—curly×wavy 78 per cent. and curly×straight 34 per cent.—which would be anticipated from Mendelian crosses of R×DR. In the third class both parents show the dominant factor and the curly-haired children constitute approximately 25 per cent.—straight×straight 12 per cent., wavy×straight 20 per cent., wavy×wavy 40 per cent., which is to be expected from DR×DR matings. The proportions are not exactly those to be expected if the straight hair is dominant, due no doubt to the small number of individuals observed, but a tendency towards dominance on the part of straight hair and recessiveness on the part of curly hair is to be noted. Some discord-

[6] Filipino Ears, this journal, Sec. A, Vol. IV. No. 1, January, 1909. pp. 27–53.

ance in the proportions may be due to the fact that when the father has curly hair and the mother straight, the curly hair is dominant, while at other times it acts as a recessive. The fact that 46 families have 47 parents with wavy or curly hair and 45 parents with straight hair, and 113 children with wavy or curly hair and 116 with straight hair indicates that either the curved or the straight may be dominant unless curly and wavy are separable forms, in which case, straight hair should be considered dominant. There are, however, twice as many known straight-haired grandparents as there are curly- and wavy-haired combined. Curly and wavy hair seem to have been latent (recessive) in the grandparents, and predominance of straight hair in the ancestry may make straight hair dominant in heredity.

Summary

There can be no doubt that hair form blends in heredity when different forms are crossed, and there can also be no doubt that segregation of hair form occurs to some extent in heredity, and one form or another may be dominant under different conditions. There is no exact conformity to Mendel's laws, although there is a tendency in that direction.

There is evident potency in the male when curly and wavy hair are crossed: when the father has curly hair, that form is dominant, and when the father has straight hair, straight is dominant. Wavy hair seems to be a blend of curly and straight in the condition of a heterozygote, each of the two forms, curly and straight, reappearing when the parents are wavy-haired. Wavy hair is epistatic to curly and hypostatic to straight; it may carry hypostatic curly while straight may carry either hypostatic curly or hypostatic wavy. There is evidence of unusual activity of the curly determiner, in spite of the fact that curly hair is usually recessive among the Filipinos.

In conclusion the results observed for Filipinos are

not the same as the results recorded by Davenport and Davenport in America but, in general, they are the reverse. When two curly-haired Filipinos are united in marriage the chances are that all their children will have curly hair. Two wavy-haired parents may have straight, wavy or curly-haired offspring. Two straight-haired parents may have children with either straight, wavy or curly hair and the proportion of straight-haired children will probably be large. When one parent has curly hair and the other straight, the greater part of the offspring will have straight hair if the father's hair is straight, but if the father's hair is curly, the proportion of curly-haired children will be larger. But the families of straight- and wavy-haired parents will probably have curly as well as straight and wavy hair, for waviness is usually heterozygous.

Note.—Wherever I may have used the construction and wording previously used by Davenport and Davenport, it was done to emphasize the fact that the results of the observations on the hair-form of the Filipino are different from those found by the two authors in America. I have reversed the terms in applying their language to the facts.

A large number of records would be desirable, both of Filipinos and of Europeans, and records of Negrito and Malay or Negro and European would be especially desirable. The hereditary reactions of different kinds of straight and curly hair may differ, but this could be determined only by more numerous records. Let us hope the records will be forthcoming.

THE ZOOGEOGRAPHY OF THE EAST INDIAN ARCHIPELAGO[1]

DR. P. N. VAN KAMPEN

TRANSLATED FROM THE DUTCH BY

THOMAS BARBOUR

TRANSLATOR'S NOTE

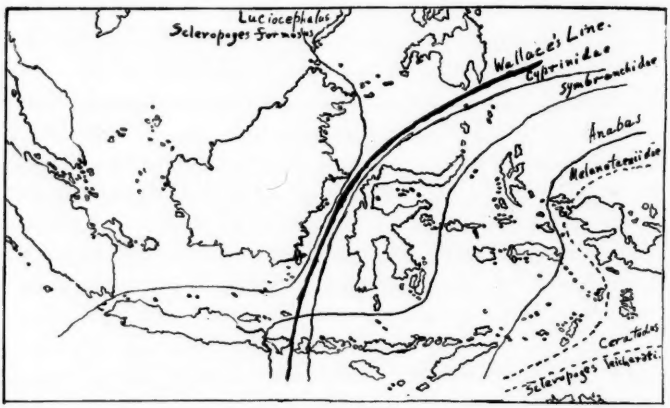
NEARLY a year ago I received from my friend Dr. van Kampen a paper which seemed at once of such present interest and general excellence that I believed it should be made available for English-speaking students of zoogeography. Its original publication in Java makes it inaccessible to many. The essay was written to be read before the Debating Club of the Batavian Royal Natural History Society; and I am under obligation to both Dr. van Kampen and the president of the Koninklijke Natuurkundig Vereeniging for permission to make and publish this translation.

THE ZOOGEOGRAPHY OF THE EAST INDIAN ARCHIPELAGO

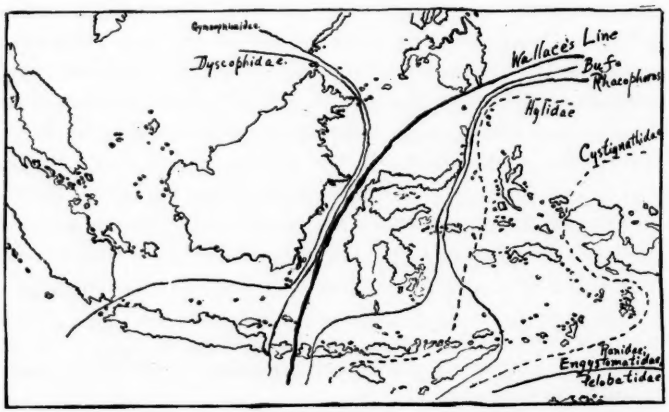
Even a superficial examination shows us that a very considerable faunistic differentiation exists between the western and eastern halves of the Indo-Australian archipelago. Perhaps this differentiation is most evident amongst the mammals. If one compares, for instance, Sumatra with New Guinea, one finds at once upon the first-named island a number of large mammals, such as the tiger and the leopard, the rhinoceros and the tapir, which are of course unknown upon New Guinea. Here, on the other hand, certain marsupials are found, as well as the strange egg-laying ant-eaters, of which there is no sign upon Sumatra. One finds similar phenomena upon

[1] Original title: "De Zoogeografie van den Indischen Archipel door Dr. P. N. Van Kampen. Overgedrukt uit Bijblad No. 3 en 4 van het Natuurkundig Tijdschrift voor Nederlandsch-Indië." Weltevreden, Boekhandel Visser & Co., 1909.

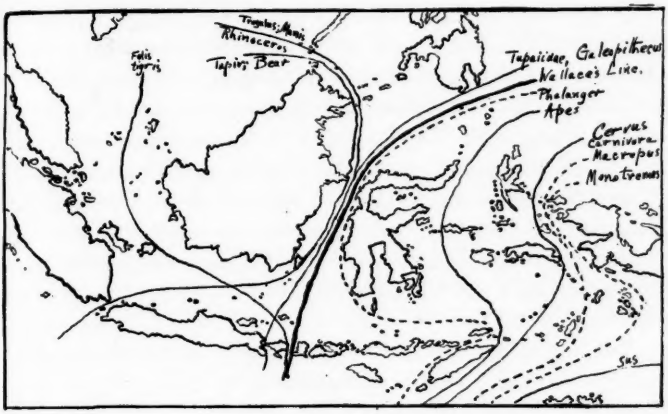
studying most of the other groups of animals. Faunistic differences comparable to these which we have here mentioned for the Indian archipelago are naturally found in other regions of the world. It is not long ago that zoologists gave little or no consideration to these phenomena of distribution, but simply busied themselves with describing hundreds of new species without caring whence these species came. It was only at the beginning of the nineteenth century that a change came about.



Freshwater Fishes.



Amphibians.



Mammals.

Explanation of the Map

On the map are given the easterly boundaries of a number of Indian groups of animals, shown by continuous line; and the westerly boundaries of some Australian forms, shown by broken lines. Map No. 1 is based on fresh-water fishes; No. 2, upon amphibians; No. 3, upon mammals. On all three maps, Wallace's line is shown as an extra heavy one; and it will be seen by comparing this with the lines bounding the ranges of other classes that it has no value as a zoogeographic boundary. The Indian animals in very many cases reach to the eastward of it, while the Australian forms do not reach out to it, so naturally do not cross it. One sees at once that the Indian and Australian components of the fish fauna are widely differentiated; and that among amphibians and mammals it is necessary to take nearly the entire eastern half of the archipelago as the transition region which we have mentioned before.

They then began to divide up the earth into a larger number of "kingdoms," by separating one fauna from another according to its similarities or differences.

These divisions differed not alone according to their originators, but also greatly in accordance with the group of animals upon which they happened to be based. Finally, it became generally agreed that Wallace, the founder of the zoogeography of the present day, had found a division which held for all land animals. That this opinion was in reality incorrect will be shown later on.

The divisions proposed by Sclater date from about 1858. Founded on the distribution of birds, it has been held by almost everybody up to the present time as covering the distribution of birds and mammals. This division of the earth is, with a few minor changes, as follows:

1. *Palæarctic Region.*—Europe; the greater part of Asia; Africa to the north of Atlas and Sahara.

2. *Ethiopian Region.*—Africa to the south of the Sahara; Madagascar and the neighboring islands; South Arabia.

3. *Indian or Oriental Region.*—India to the south of the Himalayas; south China; the western portion of the Indian archipelago.

4. *Australian Region.*—Eastern portion of the Indian archipelago; Australia; New Zealand; Polynesia.

5. *Nearctic Region.*—North America as far as northern Mexico.

6. *Neo-Tropical Region.*—Southern Mexico; the Antilles; South and Central America.

The subdivisions of these regions need not be taken up in more detail here. But it should be mentioned that Huxley in 1868 proposed the name of Notogæa for the Australian and Neo-tropical regions taken together as contrasted to the others, which he combined under the name of Arctogæa.

Zoogeography deals not alone with the question of how animals are spread over the face of the earth, but

rather attempts to explain the reasons for the peculiarities of their distribution. At first men sought the explanation especially in climatologic factors, or else they considered each zoogeographic region to be a particular center of special creation. Finally the theory of evolution in is, as in so many other subjects, spread a new light. Zoogeography has become, then, especially since it came under the leadership of Wallace, an essentially historical study.

Every type of animal has come into existence upon some specially circumscribed part of the earth, and has spread itself thence over a greater or lesser extent of surrounding territory. Only very seldom does it happen that one spreads itself over the whole earth or even a considerable part of it; the great majority are hindered by unsurmountable obstacles, and inhabit only a limited region. Zoogeography, then, on the one hand, must deal with the means of dispersal, and on the other hand with the hindrances which species may encounter.

These obstacles are, in general, spoken of as being of a climatic or orographic nature. The latter are not surprising; for if the surface of the earth were homogeneous in character, then the animals would naturally have distributed themselves in girdles or zones at even distances from the equator. Indeed, this is to a considerable extent the case with the marine fauna, against the spread of which such hindrances as we have mentioned play a minor rôle. Thus we can differentiate and recognize an arctic, a circumtropical, and an antarctic zone (Ortmann). The Indopacific Ocean, obviously belonging to the circumtropical belt, is faunistically a single entity, and to this our archipelago belongs.

While in earlier times climate and natural conditions were held as being most answerable for differences in land and fresh-water faunas, they far more slowly took the orographic factors at their true value; though naturally the first-named influences must not now remain unheeded.

Great deserts, high mountain ranges, and extensive

bodies of water are all impassable to most land animals. We can not then wonder that the fauna of North Africa is sharply differentiated from that which lies to the south of the Sahara; and so also that the Himalayas form a boundary between two zoogeographic kingdoms.

How, now, are we to explain the cases where two similar faunas are separated from each other by what seems to be a similarly impassable barrier? How is it possible, to draw an example from our own archipelago, to make comprehensible the fundamental similarity of the fauna of Sumatra and of the Malay Peninsula? In this case only two possibilities are thinkable: either by some means or other the animals have been able to get across the sea, by flying or swimming, by the aid of wind or drift-wood, or through transport by human agency; or else there has been an earlier land connection which has now completely disappeared.

Above all others this last mentioned explanation is the most fruitful for further investigation, as in general it involves calling to aid geologic factors to elucidate the reason for zoogeographic evidences of differentiation. To Wallace belongs the credit of having brought to light the true import of these factors. Zoogeography, then, may now be considered as a science auxiliary to geology.

It is evident after stating the foregoing premises that it is quite impossible to divide up the earth into sharply defined areas of distribution which hold alike for all groups of animals. Different groups owe their spread over the earth to different reasons. Some may pass easily over mountains; others (notably birds) may as easily cross the sea; some are far more dependent upon climate and the condition of the ground on which they exist than others; the oldest groups of animals, speaking geologically, have had far more time to distribute themselves than have the younger; etc. We must, however, confess that, following in the footprints of Wallace, perverted conceptions have long held sway regarding the Indian archipelago.

As to the worth of zoogeographic data in explanation of these phenomena of dispersal, we must consider the frequent impossibility of gathering all the evidence bearing on the subject. This is not the place to do more than set forth in merest generality the justification for the assertions made in the following outline. These drawbacks, moreover, surely should not hinder us from continuing, with careful circumspection, to compare most inquisitively all the available facts, being certain that many important conclusions will be reached in the end. In this manner I purpose to give here a review of that which has already been learned regarding the Indian Archipelago.

The Indo-Australian Archipelago is, from a zoogeographic point of view, a region of the highest importance. It owes this special prominence to its lying upon the boundary of two great kingdoms, the Indian and the Australian, which show a greater faunistic differentiation between one another than the rest of the old world.

Sal. Müller first noticed this difference which exists between the western and the eastern portions of the Archipelago; and, following the teachings of his time, he laid this distinction to the influence of climate and natural conditions; so that while the western half has a purely Indian character, the eastern portion—the islands of which, generally speaking, are smaller—form an area of transition to the conditions which obtain in Australia. Müller in his conclusions came in reality nearer to the present opinion than did Wallace; but the real, underlying causes of the differences remained, of course, hidden from him. The boundary between both regions he drew through the Straits of Macassar, and in the south between the islands of Sumbawa and Flores; while with some doubts he placed the island of Mindanao in the eastern, and the remaining Philippine islands in the western, section of the group.

The first to bring geologic explanations to aid in explaining the faunistic differences between the eastern and the western parts of the archipelago was Earl. But

Wallace was the foremost really to back up his opinions with valid evidence in setting forth the theory which has proved so pregnant with suggestion.

Wallace, who made long journeys among the islands, was the discoverer of the famous "Wallace's Line," as it was named by Huxley, which still, to the present day, is carefully explained in many text-books; though in reality it is disproved, and Wallace himself is not so sure of its existence during the later years of his life.

According to Wallace's original opinion, based especially upon the distribution of mammals, birds and insects, he saw a sharp faunal boundary that could be drawn through the archipelago, which ran to the eastward of the Philippines, continued between Borneo and Celebes, and on between Bali and Lombok. The fauna to the west of this line was said to be Indian; to the east, Australian. Wallace's dictum is well known, that the faunas of Bali and Lombok are more sharply differentiated from one another than those of England and Japan.

Wallace sought the explanation of these phenomena in the fact that the western half of the archipelago had in earlier times been connected with the Indian mainland, the eastern islands with Australia; and that they remained joined together until they were divided by narrow arms of the sea. The exceptions which existed Wallace explained in part through transport across water, part as their being remains of the earliest fauna which had lived upon the old land connection between Asia and Australia.

According to the researches of more recent times, among which should be mentioned especially those of Von Martens, Max Weber, and the Sarasins, it becomes evident that such a sharp boundary as Wallace drew does not exist. Not only is there none where he drew it, but no such line exists anywhere in the archipelago. Of course it is possible to draw a line which apparently bounds the distribution of some single group; and Pelseneer, upon the ground of the dispersal of molluscs, has constructed a new line which runs eastward of Celebes

and Timor, and which has been named by its author "Weber's Line." But taking the fauna as a whole it is quite certain that no line may be drawn; but, rather, we may lay out a transition zone in which the fauna of India and that of Australia are mingled, and wherein from the west to the east the Australian components increase more and more in number; and on the other hand, the Indian tend to die out. All of this region belongs to the eastern half of the archipelago from Celebes to New Guinea, and included in it we find a part of the Polynesian[2] Islands; but it is necessary to keep in mind that even the boundaries of this transition region are not sharply defined.

The justice of the position taken here will presently be sustained by some examples gleaned from among vertebrate animals, especially from fresh-water fishes, amphibians and mammals. All three groups, on account of the small likelihood of their being spread abroad over the sea, are of much importance to us.

First of all, however, the origin of the fauna of Australia merits a word. It is now commonly agreed that in past times this island formed an essential part of Asia, connected by a previously existing land-bridge which included the archipelago as it exists to-day. Formerly this connection was considered to have occurred during the Jurassic period (Neumayer's "Jura-Continent"). The finding, however, of deep sea deposits laid down in Jurassic times in different parts of the archipelago (*e. g.*, on Borneo, Celebes and Buru) has made it seem more probable that the mooted connection was delayed until the Cretaceous period; and along this connection, then, the marsupials and lung-fishes spread to Australia; as well as also the teleost, *Scleropages leichardti,* of which a near relative, *Scleropages* (= *Osteoglossum*) *formosus,* is now found upon Borneo and Sumatra, but upon no other

[2] Van Kampen uses the word *Polynesian* in a sense which is to us fundamentally wrong. He means to include the Melanesian Islands of the Bismarck and Solomon groups.

islands in the region lying between. It is not at all impossible that also a considerable portion of the present fauna of the Australian part of the Archipelago exists as a *Relictenfauna* coming down from the earliest times, although the geologic data warn us that Celebes and a considerable portion of the remaining archipelago was also covered by the sea during the Cretaceous period (Sarasin, 1901). So that, according to the latest geological evidence, it becomes apparent that in the Cretaceous period the archipelago could not all have been above water. Australia may well have received its ancient fauna from South America (Sarasin), or may equally well have had a connection with nearer India lying to the southward of the existing Archipelago (see Verbeek's *Molukkenverschlag*).

However that may have been, in any case the Indo-Australian continuity may well have been broken in Tertiary times, and thus the penetration of the Indian fauna into Australia have been brought to an end. Animals which up to that time had reached out into the archipelago would be unable any longer to reach Australia.

To such a class belong, for example, among the freshwater fishes, the family Cyprinidæ; among the amphibians, the Ranidæ; both of which are present in the Indian region by numerous representatives, while they are wanting in Australia. It is thus of importance for us to search out what may be the easterly boundary of such groups in the archipelago. Following Wallace's theory, the line which he constructed should answer this purpose. As a matter of fact the Cyprinidæ have served as one of the most important arguments for his opinion, since they did really stop at the supposed boundary line; since then, however, upon Bali we find a single pair of species of this family (*Barbus maculatus* and *Raspora argyrotænea*). During the expedition of the Siboga, Professor Weber, happened by chance to collect Rasbora upon Lombok (Weber, 1902, A); and I myself lately found the Gurami fish-ponds of the old pleasure-palace of Narmada filled with another

species of *Raspora*,[3] so that the boundary of the Cyprinidæ is thus moved to the eastward of Lombok.

Absolutely different is the real condition of the Ranidæ. This family has penetrated throughout all of New Guinea and western Polynesia; one single species, indeed (*Rana papua*), having reached the Cape York Peninsula of Queensland. Other Indian families of amphibians (Engystomatidæ, Pelobatidæ) have also reached to New Guinea. We see here then the boundaries of this transition region well defined, the westerly by the Cyprinidæ, the easterly by the Ranidæ.

Among the families of the strictly fresh-water fishes conditions are similar; and some have even reached

	Sumatra	Borneo	Java	Wallace's Line: Bali	Wallace's Line: Lombok	Timor	Celebes	Ambon	Halmahera	New Guinea	Australia
FISHES											
Notopteridæ	+	+	+	—	—	—	+	—	—	—	—
Osteoglossidæ	+	+	—	—	—	—	—	—	—	—	+
Cyprinidæ	+	+	+	+	+	—	—	—	—	—	—
Siluridæ	+	+	+	+	—	—	—	—	—	+	+
Symbranchidæ	+	+	+	—	—	—	+	—	—	—	—
Cyprinodontidæ	+	+	+	—	+	—	+	—	—	—	—
Melanotæniidæ	—	—	—	—	—	—	—	—	—	+	+
Ophiocephalidæ	+	+	+	—	—	—	+	+	+	—	—
Anabantidæ	+	+	+	+	+[4]	+	+	+	+	—	—
Nandidæ	+	+	+	—	—	—	—	—	—	—	—
Mastacembelidæ	+	+	+	—	—	—	—	—	—	—	—
Osphronomenidæ	+	+	+	+	—	—	—	—	—[5]	—	—
AMPHIBIANS											
Gymnophionians	+	+	+	—	—	—	—	—	—	—	—
Pelobatidæ	+	+	+	—	—	—	—	—	+	—	—
Hylidæ[6]	—	—	—	—	—	+	—	+	+	+	+
Bufonidæ	+	+	+	+	+	—	+	—	—	—	—
Engystomatidæ	+	+	+	—	+	—	+	+	+	+	—
Dyscophidæ	+	+	—	—	—	—	—	—	—	—	—
Ranidæ	+	+	+	+	+	+	+	+	+	+	+[7]

[3] It is entirely possible that these may have been brought in as eggs, along with the Gurami *Osphromenus olfax*, from Java.

[4] *Anabas*, sp. found by me at Ampenan.

[5] *Betta pugnax*, occurring upon Ternate according to Steindachner (1901).

[6] *Hyla dolichopsis*, recorded from Java by me (1907 A), undoubtedly introduced with plants brought into botanical gardens.

[7] Confined to the Cape York Peninsula.

farther than the Cyprinidæ. In these families one should include only those whereof all, or by far the greater majority, of the species are exclusively confined to fresh water. It is naturally evident that in other cases the possibility of their fortuitous transportation across the sea is not to be excluded. The appended table gives in outline the distribution of fresh-water fishes in the archipelago. It is gleaned, for the most part, from Weber (1894, 1897A), who has done more than any one else to add to our knowledge of the dispersal of the fresh-water fishes throughout the islands. For comparison a table is added giving the distribution also of the families of amphibians.

The eastern portion of the archipelago, including Celebes, possesses a very small fresh-water fish fauna. Those which are found most commonly in the rivers and lakes are, as Weber has pointed out, for the greater part "marine immigrants," that is to say, sea-fishes which have passed over to a fresh-water life. Of the true Indian fresh-water fishes which occur to the eastward of Wallace's line and Lombok are the Siluridæ; the New Guinea species, however, related to those of Australia and hence being in reality outside of our present range of discussion. The remaining ones are almost exclusively those which are able to withstand a drying-up—if this is not too long continued. These are, in truth, only a few species. *Haplochilus celebensis* among the Cyprinidontidæ does not get further than Celebes, nor does *Notopterus kapirat;* and two species of Symbranchidæ—*Symbranchus bengalensis* and *Monopterus javanensis*—*Ophiocephalus striatus, Anabas scandens* and *Anabas microcephalus* and possibly *Betta pugnax* are spread further to the eastward. The last-mentioned species have all a peculiar structure of the gill cavity which makes it possible for them to remain for a considerable time without water; and it has been shown by Volz for Monopterus that it may even live over through the dry monsoon without water.

The distribution of mammals in the eastern half of the Indian Archipelago is evidently not yet completely known. Even for New Guinea itself, with the exception of bats,

only a few species of mice and two of pigs (*Sus niger* and *S. papuensis*) have been discovered. Both of these last mentioned forms may perhaps have arisen from tame individuals which ran wild. Upon the Moluccas the only Indian mammals that occur are bats and mice, a single species of deer, perhaps also introduced; a few shrews, Viverridæ (*Viverra tangalunga* and *Paradoxurus hermaphroditus*), and wild pigs. Among the swine the well-known Babirusa is especially noteworthy, which is found only upon Celebes and Buru. The Moluccas (Batjan) have a single ape (*Cynopithecus niger*) in common with Celebes. But even here again it has frequently been suggested that this form was probably also introduced. In comparison with the richness in mammals of the Greater Sunda Islands, Celebes falls in with the Moluccas as showing their paucity.

The eastern half of the archipelago, in accordance with its character as a transition region, is not alone habited by immigrants which have come in from the west, but has received its fauna in part from Australia. If we take the three classes of animals which we have mentioned before, we then find here also a strong element which has spread itself even further from its origin, in this case Australia. Upon New Guinea the Melanotæniidæ among the fishes, the Hylidæ among the amphibians, and the marsupials and Monotremes among the mammals are well represented. These fishes have not dispersed themselves very far (the Melanotæniidæ not being known to have reached beyond the Aru Islands). The amphibians themselves are also almost as narrowly confined to the nearby islands, and have not even got as far westward as Celebes. The marsupials have got as far as Celebes and Timor. Although fifty-one species have been made known from New Guinea itself, so that only a small portion of these have gone farther westward; of these two have got to Celebes, both of the species belonging to the genus *Phalanger*. The fauna of New Guinea has, when one considers the Indian elements in it, about as much derived from the fauna of

Australia as that of Sumatra, Borneo and Java has derived from the mainland of Asia.

A general survey of all conditions leads to the conclusion that in post-Cretacean times there was a broad connection between the three Greater Sunda Islands and Asia on the one hand, and between New Guinea and Australia upon the other; that further also between the Sunda Islands and New Guinea a connection must have existed which was really less easy to pass over. The configuration of the bottom of the ocean supports this opinion. The western half of the archipelago is united with Asia, standing on a plateau of not more than fifty fathoms depth; New Guinea is separated from Australia by a similarly shallow sea. An elevation of the sea bottom of 45 meters would connect the Greater Sunda Islands with the mainland of Asia; while on the other hand a rise of 20 meters is all that is necessary to bring about the joining of New Guinea with Australia. The seas intervening between these two regions have, on the contrary, for the most part a great depth.

There still remains much to do in substantiation of the proof of these conclusions; and the islands and island groups of the archipelago are still a fruitful field of inquiry for those who may be interested.

As already remarked above, Sumatra, Java and Borneo lie upon a shallow submarine plateau which binds them to further India. Upon this same plateau lie also a host of lesser islands such as Banca and Billeton, Madura and Bali. All of these islands, as well as those which lie along the west coast of Sumatra, and which are surrounded by a sea of considerably greater depth, possess a fauna which in all its principal characteristics is essentially that of the Malay Peninsula. Here alone one finds the great Indian mammals, such as the Orang Utan, the tiger, the leopard, the Malayan bear, elephant, tapir, rhinoceros and bantang. Even still more evident is the relationship of the mainland when one takes into account the finds among the fossils. Dubois has uncovered a Tertiary fauna upon Java that bears the most marked resemblance to the Pliocene Siwalik

fauna of Hindustan. Here he found, for instance, remains of Hippopotamus, Stegodon, a species of antelope, Hyæna, Gavialis, etc.

Taking the present condition alone into account, Sumatra and Borneo more than the others have a fauna that is almost identical with the fauna of the Malay Peninsula. Java shows a greater difference; whereupon one may draw the conclusion that it broke off earlier than the other islands from the mainland, since between Java and Sumatra there was more migration than between Java and Borneo, so that it was separated earlier from the last mentioned island than from Sumatra. The Javan Sea, as the Sarasins have determined from a study of the land snails, is of very ancient date.

On the other hand, Java has some mammals, *Rhinoceros sundaicus, Trajulus stanleyanus, Viverricula malaccensis, Helictus orientalis,* in common with the mainland, which up to the present have not been found upon either Sumatra or Borneo. So far, for this state of affairs absolutely no satisfactory explanation has been found.

We might also say of Borneo that it has received a small portion of its fauna evidently by way of Palawan and the Sulu Archipelago from the Philippines, and so directly from south China, and indeed from the Himalayan region itself.

Celebes has always been the great bone of contention from a zoogeographic point of view. We have already seen that Sal. Müller and originally Wallace considered it as belonging to the Australian half of the archipelago. Now, especially through the labors of Max Weber and the Sarasins, it has been made evident that its fauna is essentially Indian, even though it may be mixed with a few Australian types.

But even so, the fauna of Celebes is as yet only partly elucidated. We find here an astonishing number of peculiar types which no one has been able to find upon the other larger islands of the archipelago; and so there must always be a question as to the origin of its fauna.

The first peculiarity is the occurrence of a number of

animals which find their nearest relatives not in the further Indian Archipelago, nor yet even upon the continent of Asia, but in Africa itself. Taking the number of these species of animals together, we must conclude that they are of very ancient origin indeed. Among the birds which must be grouped in this way are such species as *Coracias temmincki* and *Cittura,* the most primitive kingfisher; among the mammals two species of monkey, namely *Macacus maurus,* and its next of kin, *M. arctoides,* which belongs in Hindustan, and *Cynopithecus niger,* most closely related to the African baboons;[8] in addition, the strange *Babirusa,* a pig that is more like the African warthogs than any other variety of swine. The dwarf-buffalo, or Anoa (*Bos depressicornis*) may also be considered as a relic-species, of which its only near living relative, *Bos mindorensis,* inhabits the Philippines.

The explanation of this relic-fauna the Sarasins take to be an old invasion evidently spread out from Java, which most likely happened in the Miocene period. It can not have taken place earlier, since the same investigators were astonished at the geologic evidences that Celebes in Eocene times was wholly covered by the sea. The more recent fauna (that which shows itself among mammals in the abundance of species of squirrels) arrived later, and in all probability entered Celebes along the same land connection. This explanation is not entirely satisfying, since it does not make evident why these animals that we have mentioned exist on Celebes, but have died out everywhere else. On the other hand, that such things really can occur is proved by the abundance of the finds of fossil remains of animals in places where they no longer exist. The epoch-making discoveries of Dubois are of weighty import in this connection.

A further peculiarity of the fauna of Celebes is its great difference from that of Borneo. The Sarasins

[8] Matschie brings the monkeys of Celebes together into one genus (*Papio*), while he differentiates a number of new local races upon the same island. Trouessart, in the supplementary part of his "Catalogus mammalium," has united all these forms as varieties of one single species.

have announced that both islands have not one single species of animal common to them which is not also found upon Java, Sumatra or the Philippine Islands. There is not the slightest possibility of there having been a direct land bridge between Celebes and Borneo across the Straits of Macassar since the very earliest geologic times. This difference between Celebes and Borneo is beyond doubt one of the real reasons for the unjustifiable opinion of Wallace and for the placing, as he did, of his boundary line.

That the Java sea is, according to the Sarasins, of great age is shown by the fact that a curved line may be drawn through this sea and continued into the Straits of Macassar, which terminating blindly, so to speak, at both ends, cuts the archipelago into two portions, or, we might better say, bounds one part where the islands of younger geologic age have apparently had no connection with the mainland. It has thus an entirely different significance from the boundary line as it was formerly drawn.

A third peculiarity of Celebes upon which Weber has laid particular stress is the paucity of certain groups of animals in comparison with the three Greater Sunda Islands. Weber has considered the fauna of Celebes as being essentially an impoverished Indian one. This poverty appears best brought out by the appended table,[9] wherein the comparison as regards the approximate extent of the islands is given.

	Sumatra	Borneo	Java	Celebes
Surface in sq. km	431,000	733,000	126,000	179,000
True fresh-water fishes[10]	212	202	131	4
Amphibians	50	78	37	25
Reptiles	175	191	129	87
Mammals (exclusive of bats)	115	133	68	39

[9] These data in this table are drawn for the most part from Weber (1894), Popta (1905–6), and Volz (1907), for the fishes; Van Kampen (1907 B), for the amphibians; Sarasin (1901) for the reptiles; Tjeenk Willink (1906) and Schneider (1905), for the mammals.

[10] These are drawn from the same families that were mentioned in the table upon page 547.

This difference is noticeable among the fishes above all others. On the other hand, the difference is less among the birds, and the opposite holds for land and fresh-water molluscs whose number is reckoned by the Sarasins for Sumatra as 129 species; Borneo, 311; Java, 233, and Celebes, 238.

Weber explains the poverty of fauna in this manner: that Celebes, as we understand it to-day, has not long been in its present condition, but rather that it was formed by the uniting of a number of small islands which, as is always the rule in such cases (especially among the fresh-water fishes) possess a sparse or poor fauna. In this same wise is it made evident why the fresh-water mussels (Unionidæ) which occur upon the continent of Asia and Australia, and upon the Greater Sunda Islands, are lacking on Celebes. Von Martens comes to this same conclusion, that they (along with other generally distributed families of fresh-water molluscs) do not occur upon small islands. They are wanting thus upon the Moluccas and on Celebes, where the entire fauna has been made up by the merging together of those of several smaller islands to form a single one.

The opinion of the Sarasins is somewhat different from the explanation of Weber. They are of the opinion, brought forward as especially important, that between the different parts of the island ancient faunistic differences are demonstrable, a peculiarity of the fauna of Celebes which has been thoroughly investigated by them. The Sarasins have taken this up especially from the point of view of distribution of land and fresh-water molluscs. Von Martens had already noticed that north and south Celebes possessed hardly a single species of mollusc in common; and the Sarasins made evident that there existed an easterly mollusc fauna, besides a well-differentiated fauna in the great lakes of central Celebes. Other animals exist, divided from each other in this same manner. So among the mammals, the Babirusa and the crested baboon, *Cynopithecus,* are found in the northern

portion of Celebes only; as is also that remarkable bird *Megapodius cumingi,* as well as some others.

Another point emphasized is the fact that the molluscs of the different peninsulas of Celebes show closest relationship with the island lying near them. The Sarasins found among the groups of animals which they studied that the species which have survived and which have become peculiar now to Celebes, as well as those which have a wider spread at the present time through the archipelago, show in part a relationship to species from Java, in part to those from the Philippines, or from the Moluccas, or again from the Lesser Sunda Islands, especially Flores. The proportion of these four components of the fauna of Celebes is about the relationship of 4:3:2:1. Here again is emphasized the overwhelmingly Indian characters of Celebes, since alone among these last two mentioned smaller groups no Australian species have been found. A close relationship with Borneo is, as we have mentioned previously, entirely non-existent.

The explanation of the Sarasins is this. Celebes rose from the sea in Eocene times, and in Pliocene times especially received its fauna along four land bridges, each of which connected the island with one of the previously mentioned islands or island groups. The position of these bridges is still traceable through submarine shallows, or else by groups or chains of islets. The Java bridge ran from the southern peninsula out through what are now Postillon, Paternoster and Kangean Islands, to eastern Java. The Philippine bridge bound the northern peninsula with Mindanao, and included the present Sangi and Talaut groups. The Moluccan bridge went off from the eastern peninsula; and united together the Peling and Sula Islands, and apparently then split up into two bridges, one of which ran off to the Obi and Halmahera groups, the other to Buru. This last connection is postulated by the geographic distribution of the Babirusa, and also by the birds. The latter, from the mountains of Buru, show a close relationship with Celebes. However, during the expedition of the Siboga,

the strait between Buru and the Sula Archipelago was found to be of considerable depth (about 4,100 meters), an argument against any such land connection.

It occurs to me that the past events which have been reconstructed by the Sarasins are just as well explained by Weber's opinion, which is that the separated islands, which gave rise to Celebes, by consolidation, have received their fauna in the way which the Sarasins have suggested. The point of difference is indeed simply a question of interpreting the significance of the earlier history. In either case it is entirely plausible that Celebes has received its fauna from the south,[11] the north, and the east; not directly from Borneo, but rather by means of small islands or narrow land bridges; which fact has had a great influence on the impoverishment of the fauna. On the other hand, animals from Celebes have been enabled to spread out along these same land connections, so that occasionally we find evidences among the Philippines and the Lesser Sunda Islands of this having happened by their having certain common animal types.

It is peculiar that the truly Indian character of Celebes (the great westerly island of the transition region) remained unsuspected for so long; while on the other hand, no one doubted, but rather laid stress upon, the Australian relationship of that vast easterly island, New Guinea, the fauna of which is fully as Indian as that of Celebes is Australian. It simply happened that those groups of animals which at once were most evident, and which had been most frequently used in elucidating zoogeographical questions—especially birds and mammals—are preponderatingly Australian upon New Guinea, and happen to show upon Celebes also a considerable Australian admixture. Had earlier investiga-

[11] Weber brings into existence two different land bridges as an argument to explain the difference between the fauna of Java and Flores. He is inclined to the opinion that there was here a land mass of considerable extent which has given rise to the great depths of the Flores Sea by an extensive sinking.

tors laid more stress upon the amphibians, then an entirely different result would have been arrived at: New Guinea possesses here again 39 species which belong to Indian families as against 24 Australian.[12] Among other groups of animals New Guinea is more Indian than Australian. This is the case with the scorpions and the earthworms.

New Guinea, then, has a mixed fauna, whereof the Australian elements are explained by the existence of a now broken connection with Australia; and whereof the Indian elements evidently arrived from the Moluccas. The Sarasins have constructed, in explanation of this, two bridges, one of which connected New Guinea with Halmahera—the New Guinea bridge of Kuckenthal (1903); the other, with Ceram. The Aru and Ke islands range themselves along with New Guinea. Upon both, for instance, we find a true kangaroo, *Macropus brunii* [*sic*], an animal which does not occur further to the west of New Guinea. The Aru Islands possess a fresh-water fish of the family Melanotæniidæ, the remaining members of which are confined to Australia and New Guinea (Weber, 1907B). This same relationship is shown by birds such as crown-pigeons, birds of Paradise, and cassowaries, as well as among amphibians and other groups. One can, therefore, take it for granted that at the same time as New Guinea both of these two island groups were united with Australia, although the Ke Islands are cut off by a greater depth of sea.

From the already mentioned faunistic difference between Celebes and New Guinea, it follows that for many families and genera of animals both the westerly as well as the easterly boundaries must lie in the Moluccas. Here one would expect to be able to place a boundary line which would exactly fit each special case. But here we are hindered further by the fact that just these is-

[12] In this are included the species described by me in a memoir the preliminary of which will appear in Nova Guinea. See also Van Kampen (1906). [The paper in "Nova Guinea" has already been published.—Translator.]

lands are the ones which are still only incompletely known. With the exception perhaps of certain groups of animals—birds, butterflies, etc.—which on account of their value are specially sought for, there are great gaps in our knowledge. Only during the last few years have Halmahera, Ternate and Batjan been systematically studied by Kuckenthal. These islands then are the best known. Obi, Buru, Ceram, the Sula Archipelago and others are no more than zoologic blanks upon the map. It is a fact that upon the Sula Islands a short time ago 19 species of land snails were collected, while only a short time before only one single species was known (Schepman). These last-mentioned islands are of importance as having belonged to the Molucca bridge of the Sarasins.

Another question is whether the Moluccas have received their Indian fauna along this Molucca bridge from Celebes, as the Sarasins claim, or along still a different way, as perhaps through the Talaut Islands from the Philippines, as some cases tend to show.

These questions must wait for a definite answer until researches have brought more light to bear upon them. Before this we can only go so far as to say with certainty that the fauna of the Moluccas is a mixed one, as is that of New Guinea; and that in large part on account of the generally small size of the islands both the Indian and Australian components are found impoverished in comparison with the neighboring greater islands.

Hardly more complete is our knowledge of the Lesser Sunda Islands. Weber (1902) placed them among the very least known of any in the archipelago. Here remain a number of knotty questions still to be explained, although it has been definitely proved that the sharp line which Wallace drew between Bali and Lombok must be given up for good and all. The existence of one of the most fundamental supports of this boundary was disproved by the expedition of the Siboga; and that was regarding the great depth of the Lombok Straits. Indeed, this strait is somewhat deeper than that between the other

islands of the series from Bali to Ombaai; and the Sarasins believe that it was in reality one of the oldest, although more recent than the Strait of Macassar, and thus well within the Secondary Epoch. This allows us to explain a certain faunistic differentiation between Bali and the other islands; for we know, for instance, that cockatoos do not occur further to the westward than Lombok. This was one of the weightiest arguments which Wallace used. However, cockatoos live in the Philippines, and thus well to the westward of his own line. Weber points out that the fauna of the Lesser Sunda Islands is in large part an impoverished Indian one derived from Java, beside which there occurs a small element from Celebes (along the Flores bridge). So also by the same means has come a slight infiltration of Philippine forms. Here also we find a few Papuasian or Australian species. Timor appears, indeed, to have received some animals directly from New Guinea or Australia (birds, *Hyla*).

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SHORTER ARTICLES AND DISCUSSION

BIOMETRIC ARGUMENTS REGARDING THE GENOTYPE CONCEPT[1]

Some . . . persons vainly seek by dialectics and far-fetched arguments, either to upset or establish things that are only to be founded on anatomical demonstration, and believed on the evidence of the senses. He who truly desires to be informed of the question in hand, and whether the facts alleged be sensible, visible, or not, must be held bound either to look for himself or to take on trust the conclusions to which they have come who have looked; and indeed there is no higher method of attaining to assurance and certainty.—William Harvey, *Second Disquisition to John Riolan, Jun.*

In a recent number of this Journal[2] Dr. J. Arthur Harris has published a general attack on the pure-line or genotype concept of inheritance. In the course of this paper its author sees fit to criticize certain work done by the present writer and his former colleague, Dr. Frank M. Surface. These criticisms in the main seem to me, upon analysis, to rest on either a misconception of what our results actually are, or else a lack of understanding of the real facts regarding certain of the biological points involved. If this is the correct view, it means that our presentation of pertinent data has been either obscure or incomplete or both. It is the purpose of this note to endeavor, if possible, to remedy this defect in some degree at least.

The points made by Harris in criticism of the work done in this laboratory may be considered *seriatim*.

1. In a section entitled "Characters which are not Inherited at all can not be Taken to Prove that Selection in General is Ineffective," Harris very cautiously avoids the direct statement that fecundity in fowls is not inherited, but since about one half of the section (pp. 353–356) is devoted to a discussion of the work of the Maine Station on this matter, it is not unfair to suppose that he is of opinion that this work illustrates and exemplifies the dictum which stands as the title of the section. He concludes in a later section that the present writer's investiga-

[1] Papers from the Biological Laboratory of the Maine Experiment Station, No. 28.

[2] Amer. Nat., June, 1911.

tions indicate "that the high-laying mothers tend to produce low-laying daughters; selection to increase egg production actually decreases it."

Definite numerical data tending to show that fecundity in the domestic fowl *is* inherited has been presented by the writer in the June, 1911, number of the AMERICAN NATURALIST (pp. 321–345). It obviously need not be repeated here. Any interested person may examine the data and draw his own conclusions as to whether "selection to increase egg production actually decreases it," when this selection takes the form of isolating genotypes of high fecundity.

2. The second point made by Harris is against what was intended to be a popular bulletin on sweet corn[3] in the introduction to which the following statement was made: "It is the purpose of this bulletin to give a brief account of the general features of the experiments carried on during the past three years in sweet-corn breeding. Further discussion of the technically scientific results of this work is reserved for later publication." No such publication has appeared.

The criticism of this bulletin is included in a section headed "Improvement for any Single Character can not be Supposed to be Unlimited." The facts are these: In 1907 selection was begun for earliness in an excellent strain of sweet corn. A *marked* gain was made in the quality after one year's selection. No further gain has been made though selection has been continued.

It is Harris's contention that probably after one year's selection the strain had reached its physiological limit in earliness, and that the result obtained can not be urged against the biometric theory respecting selection, nor in favor of the genotype concept. He further gives the reader the impression that Dr. Surface and I regarded these results on earliness as having an important and critical bearing upon theories of selection and inheritance. For the benefit of those readers who do not follow agricultural literature it may be well to quote the final conclusion (numbered 14) printed on p. 307 of the bulletin Harris criticizes:

> No attempt is made at present to discuss the biological basis of the improvement in earliness observed to follow selection for that character in these experiments. *We are inclined to the belief that much, if*

[3] Me. Agr. Expt. Station, Bulletin 183.

not all, of this improvement is in reality a physiological rather than a genetic or hereditary phenomenon.[4] The whole subject of breeding for earliness is one which needs more critical discussion and experimentation than has hitherto been given it.

The same opinion is expressed in the body of the bulletin (p. 302). How one could take a more emphatically agnostic position than this regarding the interpretation of the results of the selection of maize for earliness is not evident.

In regard to characters other than earliness, such as ear conformation, fineness of grain, yield, etc., which Harris does not mention in his critical remarks, it was and is our conclusion that the results in hand indicated that these characters were not inherited in accordance with the so-called "law of ancestral inheritance," but that our results with these characters gave support, so far as they went, to the genotype concept, thus agreeing with the findings of other students of maize. Whether from too hasty reading of the bulletin criticized or some other reason, Harris has made it appear in his paper that we had earliness in mind in drawing this conclusion. I trust that our position is now clear.

Harris assures us (p. 359) that he has "no malicious desire to differ" from us. This it is a pleasure to know for two reasons: one is that a person would dislike to think that he was the butt of malicious and persistent personal attack from a scientific worker; the other is that since there is no intention on his part to be personal and since ours is the *only* work on maize with which he finds fault or feels it even necessary to mention, it must mean that he finds nothing to object to in the only critical, analytical evidence on genotypes in maize which, so far as the writer is aware, has ever been published, namely, that of East and Shull. It is a distinct gain thus to find that there is one body of evidence in favor of the genotype concept which its critics unreservedly accept.

3. It is suggested by Harris that 200 eggs per year represents about the attainable limit of egg production in fowls, and that the reason no improvement was made in our selection experiment was that this physiological maximum had been reached. Here two points need to be made plain. The first is that 200 eggs per year is *not* the physiological maximum of egg production in the domestic fowl. As evidence here the case of a pullet

[4] Not italicized in original.

which laid over 300 eggs in a year may be cited. While the case did not occur in the poultry yards of this station, there is no doubt as to its correctness. Furthermore, the case is by no means an isolated one, similar records having been obtained under controlled conditions in other parts of the world. This record was made by a cross-bred Orpington pullet in New Jersey. The history of her laying is as follows: She began laying on November 20, 1909. Up to August 25, 1910, she laid practically continuously, producing in that period 275 eggs. She then went broody, and in late September and October moulted. The September record was 13 eggs; the October record 12 eggs, and up to November 9, 6 eggs, making a total production of 306 eggs in 11 days less than a full year.

Further evidence of the same sort is furnished by the report of the last Queensland (Australia) laying competition which has just come to hand.[5] In this competition 150 pullets took part, comprised in 25 pens of 6 birds each. The aggregate egg production of 12 months, from April 1, 1910, to March 31, 1911 (the natural "laying year" in the southern hemisphere), was 31,165, or an average per bird, taking the *whole lot together*, of 207.77 eggs. The three highest pens (of 6 birds each) made average records per bird of 253.33 eggs, 238.83 eggs and 218.67 eggs, respectively.

The second point is that the Maine Station *flock* never attained an *average* production of 200 eggs per bird, or anything like it. But it can not be supposed that a *physiological limit of selection* for production is reached until the *mean* egg production per bird for the *flock* has reached about the physiological limit of the *individual*. The point apparently not clearly grasped by Harris is this: for a long period of years prior to 1898 when the mass selection experiment at the Maine Station was begun, the strain of birds with which it was begun had been under the observation of the man who inaugurated the experiment, the late Professor Gowell. The mean annual production of this flock had been, according to his statement, which there is no reason to doubt, about 125 to 135 eggs per bird. Now there were then known and are now known many flocks of birds which produce an average of 150 to 160 eggs per bird per annum. To attempt to raise the average production of the Maine Station *flock* from say 130 to say 150 eggs per bird per annum by mass

[5] *Feathered Life*, Vol. 12, p. 450, 1911.

selection could hardly by any stretch of the imagination be considered to be approaching the physiological limit of fecundity of the fowl. This is precisely what was attempted during a period of eight years, and was not accomplished. While there is no doubt in all organisms a physiological limit beyond which selection of any kind can not carry the strain or race, such a limit was most certainly never even remotely approached in the course of the mass selection experiment at the Maine Station.

4. Harris emphasizes by the use of italics the fact that the only correlation coefficients between mother and daughter in respect to egg production which I have ever published, while sensibly equal to zero having regard to the probable errors (with a single exception where daughter averages are used), yet deviated in the *negative* direction[6] when they departed from zero at all. Lest some one should attach some importance to this circumstance it may be said that these coefficients to which he refers cover only one year's work (1907–08). Since that time I have calculated the coefficients of correlation in respect to fecundity between mother and daughter and grandmother and granddaughter for the years 1908–09 and 1909–10, for various age classes of mothers, etc. These correlations involve over 1,000 daughters and over 200 mothers. *All of these correlations are sensibly zero,* having regard to their probable errors. Of 11 such coefficients 8 deviate from zero on the positive side and 3 on the negative side. Putting the 1907–08 published coefficients with these it gives, out of 16 coefficients, 10 deviating from zero in the plus direction and 6 in the minus direction. Certainly it is difficult to see how this could be made to indicate that "high-laying mothers tend to produce low-laying daughters."

By way of conclusion it may be said that the writer hopes that what is here set forth will make clear the facts regarding the points criticized by Dr. Harris in the work from the biological laboratory of the Maine Station. So far as concerns his argumentation respecting the genotype concept as a whole, or his personal opinion of the critical value of the work done in this laboratory no discussion will be entered upon by the present writer. Since the merits of both cases rest upon nothing

[6] Harris implies that this was true of *all* 1907–08 correlations. This is not so, as any one may see by turning to p. 71 of Bulletin 166, where of the five coefficients published, and these are *all* which have been published up to the present time, three are negative and two are positive.

but an accumulation of plain, unadorned facts available to any one's inspection, it seems useless to try to bolster either of them up by the dialectic methods of a lawyer's appeal to the jury.

RAYMOND PEARL.

June 21, 1911.

ON THE FORMATION OF CORRELATION AND CONTINGENCY TABLES WHEN THE NUMBER OF COMBINATIONS IS LARGE

IN earlier numbers of this JOURNAL two papers on that useful tool, the correlation coefficient, have appeared. The first[1] explains and illustrates a convenient method of carrying out the arithmetical routine of calculation, while the second, by Professor Jennings,[2] describes a method for obtaining the coefficient for symmetrical tables without the labor of actually rendering the tables themselves symmetrical.

The purpose of this note is to point out a method of preparing correlation tables where the number of combinations is large. Such tables are not infrequently needed. Suppose, for

[1] Harris, J. Arthur, "The Arithmetic of the Product Moment Method of Calculating the Coefficient of Correlation," AMER. NAT., Vol. 44, pp. 693–699, 1910.

In a note on this method of calculating the coefficient of correlation, Professor Jennings (AMER. NAT., Vol. 45, p. 413, 1911) suggests reduction in size of the moments by designating the lowest grade by 0 and the succeeding ones by 1, 2, 3, ... n. In this he is quite justified. I have frequently used the scheme he suggests during the last several years, but I did not refer to it particularly in my note, and for two reasons. First, I thought the point sufficiently covered by the statement that the rough moments may be taken about any arbitrary point as origin, and by the suggestion that when the range is very great it may pay to use the conventional methods in calculating the standard deviations. Second, according to my experience it is better, whenever possible, to keep the actual values. When one uses a mechanical calculator the arithmetical routine is (after a little practise) not out of proportion to the advantages. Under many circumstances these are very great: (a) all the values have a direct biological (physical) significance, (b) the means of arrays may at once be obtained for testing linearity of regression, (c) tables for different lots of material may be combined or separated at will by merely summing or segregating their moments, and finally (d) I shall show in a forthcoming paper how these moments, once calculated, may be of much service in obtaining some of the more difficult correlations.

[2] Jennings, H. S., "Computing Correlation in Cases where Symmetrical Tables are Commonly Used," AMER. NAT., Vol. 45, pp. 123–128, 1910.

instance, that one wishes to correlate between the hatching quality of the eggs of sisters in the domestic fowl, as Pearl and Surface[3] have actually done. If each family be composed of only ten pullets and there be only fifty families the number of entries in the symmetrical correlation table will be $10 \times 9 \times 50 = 4{,}500$. Or again, if one be interested in determining whether the dimensions or proportions of the blood corpuscles differ from individual to individual in an animal, say the tadpole,[4] and have measurements of twenty-five corpuscles in each of 100 individuals, he may have to form a correlation table of 60,000 entries. Much larger tables than this have been formed. The labor is of course excessive, and this has been one of the factors limiting their application to problems of morphology, physiology and heredity.

In many cases the routine, as I have found from considerable experience, can be profitably carried out as follows.

The individuals of each class are seriated separately and the frequencies entered in horizontal rows in a table of vertical columns, each devoted to one of the grades of variates, g_{m+1}, g_{m+2}, . . . g_{m+n}. A second table, exactly like the first in width of column and row is prepared and cut into strips by columns. Each of these columns is moved successively across the surface of the original table, and the frequencies which are in juxtaposition are multiplied together and their products summed and entered on a correlation blank, in the compartment corresponding to the captions of the two columns. This is repeated for all the columns *except the one identical with the strip.* If the strip be for grade g_{m+2}, the multiplications and summations from once passing it over the original table give the whole relative array associated with it as subject, except the frequencies of the diagonal cell, $g_{m+2} - g_{m+2}$.[5] To obtain these each frequency on a strip is multiplied by itself less one and the products summed.

It is not absolutely necessary, since the table is symmetrical,

[3] Pearl, R., and Surface, F. M., "Data on Certain Factors Influencing the Fertility and Hatching of Eggs," Bull. Me. Ag. Exp. Sta., No. 168, pp. 147–151, 1909.

[4] For actual cases, see K. Pearson, "A Biometric Study of the Red Blood Corpuscles of the Common Tadpole (*Rana temporaria*) from the Measurements of Ernest Warren," *Biometrika,* Vol. 6, pp. 402–419, 1909.

[5] Diagonal cell is the term applied to a compartment of a row extending diagonally across the correlation surface. In symmetrical tables they contain the frequencies for identical values of the subject and relative.

to obtain the products of the frequencies of all of the columns by all the strips, but by doing so a check is obtained for all entries except those of the diagonal cell.

The great advantage of this method is that it replaces mental and pencil drudgery with rapid mechanical calculation. Clipping the movable column by the side of the one with which it is to be compared in the table, one can obtain the products and the sum of the products simultaneously on a Brunsviga,[6] by merely multiplying the successive pairs of frequencies together and allowing the products to accumulate. Of course the frequencies for the diagonal cell can be quickly obtained, by summing the $n(n-1)$ values for the individual column, in precisely the same manner.

Purely as an illustration of method the intra-individual or homotypic correlation for number of seeds developing per pod in a series of Broom plants (*Cytisus scoparius*)[7] collected at Woods Hole in the late summer of 1907 will now be determined.

TABLE I

SEEDS PER POD FOR TWENTY-THREE INDIVIDUAL PLANTS

	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
1	–	–	2	2	1	9	7	10	9	15	9	11	4	3	8	3	3	3	1	–	–	100
2	–	–	—	—	—	—	1	3	3	6	12	6	14	13	12	5	5	11	5	3	1	100
3	–	–	—	—	1	—	2	4	2	4	10	11	7	14	11	12	13	7	2	–	–	100
4	–	–	—	—	—	5	3	7	12	15	14	16	16	7	3	2	—	—	—	–	–	100
5	–	1	—	1	1	4	5	8	10	10	13	12	10	18	4	2	1	—	—	–	–	100
6	–	–	7	7	7	12	8	11	8	8	7	9	7	7	2	—	—	—	—	–	–	100
7	–	–	—	1	1	7	2	5	5	10	7	16	7	9	10	9	2	8	1	–	–	100
8	1	1	2	1	4	3	4	9	8	8	16	9	9	14	7	4	—	—	—	–	–	100
9	1	–	—	—	1	7	9	4	7	11	18	12	10	7	4	6	3	—	—	–	–	100
10	–	–	2	1	3	5	8	9	7	6	14	11	12	10	6	5	1	—	—	–	–	100
11	–	1	2	2	4	3	8	8	8	8	10	13	11	6	5	5	3	3	—	–	–	100
12	–	–	2	1	6	10	17	17	15	10	11	4	6	1	—	—	—	—	—	–	–	100
13	–	–	—	1	—	1	2	3	12	14	12	14	10	12	12	3	3	1	—	–	–	100
14	–	–	—	1	5	5	9	6	11	11	9	9	11	6	9	5	2	1	—	–	–	100
15	–	–	—	—	—	5	3	6	5	4	8	10	8	8	13	13	9	5	3	–	–	100
16	–	–	1	—	1	7	6	7	7	7	9	8	8	8	8	9	8	4	2	–	–	100
17	–	–	—	1	1	8	9	10	7	11	10	14	13	12	3	1	—	—	—	–	–	100
18	–	–	—	—	—	2	1	3	6	12	15	13	13	7	15	8	4	1	—	–	–	100
19	–	–	—	—	1	5	5	7	15	12	11	14	13	11	4	2	—	—	—	–	–	100
20	–	–	—	3	4	6	10	12	6	6	12	10	11	6	6	3	3	2	—	–	–	100
21	–	–	2	2	1	1	4	4	8	5	11	13	10	9	7	3	3	1	—	–	–	84
22	–	1	2	5	5	13	8	8	6	13	5	6	6	2	—	—	—	—	—	–	–	80
23	–	–	2	14	12	11	11	9	6	7	1	1	—	—	—	1	—	—	—	–	–	75
	2	4	24	43	59	129	142	170	183	213	244	242	216	190	149	101	63	47	14	3	1	2,239

[6] A Comptometer will also do.

[7] The variability of these has already been compared with that of Pearson's English series. See "Variation in the Number of Seeds per Pod in the Broom, *Cytisus scoparius*," AMER. NAT., Vol. 43, pp. 350–355, 1909.

TABLE II

INTRA-INDIVIDUAL OR HOMOTYPIC CORRELATION FOR SEEDS PER POD IN THE BROOM

	0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	
0		1	2	1	5	10	13	13	15	19	34	21	19	21	11	10	3					198
1	1		6	9	14	23	25	33	32	39	44	40	36	40	16	11	4	3				376
2	2	6	58	105	122	201	196	232	197	207	212	207	173	147	88	51	28	18	4			2,254
3	1	9	105	256	287	390	374	390	313	373	276	300	240	195	123	74	36	30	3			3,775
4	5	14	122	287	286	477	536	559	482	511	489	446	412	320	208	147	70	48	6			5,425
5	10	23	201	390	477	898	1,009	1,130	1,066	1,268	1,204	1,227	1,049	824	586	424	223	166	45			12,220
6	13	25	196	374	536	1,009	1,066	1,284	1,230	1,324	1,435	1,328	1,220	943	649	458	265	161	39	3	1	13,559
7	13	33	232	390	559	1,130	1,284	1,338	1,446	1,581	1,749	1,677	1,513	1,267	885	598	351	253	70	9	3	16,381
8	15	32	197	313	482	1,066	1,230	1,446	1,516	1,835	1,990	1,965	1,762	1,470	1,065	656	365	240	62	9	3	17,719
9	19	39	207	373	511	1,268	1,324	1,581	1,835	2,008	2,296	2,330	2,038	1,694	1,320	814	457	345	89	18	6	20,572
10	34	44	212	276	489	1,204	1,435	1,749	1,990	2,296	2,648	2,708	2,489	2,189	1,671	1,104	669	462	138	36	12	23,855
11	21	40	207	300	446	1,227	1,328	1,677	1,965	2,330	2,708	2,612	2,448	2,170	1,684	1,128	671	494	125	18	6	23,605
12	19	36	173	240	412	1,049	1,220	1,513	1,762	2,038	2,489	2,448	2,094	1,956	1,488	959	575	442	135	42	14	21,104
13	21	40	147	195	320	824	943	1,267	1,470	1,694	2,189	2,170	1,956	1,812	1,397	928	598	458	145	39	13	18,626
14	11	16	88	123	208	586	649	885	1,065	1,320	1,671	1,684	1,488	1,397	1,228	919	618	480	155	36	12	14,639
15	10	11	51	74	147	424	458	598	656	814	1,104	1,128	959	928	919	640	506	361	118	15	5	9,926
16	3	4	28	36	70	223	265	351	365	457	669	671	575	598	618	506	356	275	99	15	5	6,189
17		3	18	30	48	166	161	253	240	345	462	494	442	458	480	361	275	254	103	33	11	4,637
18			4	3	6	45	39	70	62	89	138	125	135	145	155	118	99	103	30	15	5	1,386
19							3	9	9	18	36	18	42	39	36	15	15	33	15	6	3	297
20							1	3	3	6	12	6	14	13	12	5	5	11	5	3		99
	198	376	2,254	3,775	5,425	12,220	13,559	16,381	17,719	20,572	23,855	23,605	21,104	18,626	14,639	9,926	6,189	4,637	1,386	297	99	216,842

The original data appear seriated by individual plants in Table I. From this we derive the symmetrical intra-individual correlation surface, Table II.

Working by the conventional product moment method, but taking all moments around 0 as suggested elsewhere in these pages,[8] we get:

$$\begin{aligned} \Sigma(x') &= 2{,}179{,}781, & \nu_1' &= 10.0524 = A, \\ \Sigma(x'^2) &= 24{,}578{,}235, & \nu_2' &= 113.346284, \\ \Sigma(x'y') &= 22{,}438{,}814, & \mu_2 &= 12.295679 = \sigma^2, \\ N &= 216{,}842. & \Sigma(x'y')/N &= 103.480018. \end{aligned}$$

The y moments are of course the same as the x, and we have:

$$r = \frac{S(xy)}{N\sigma_x\sigma_y} = \frac{S(x'y')/N - \nu_x'\nu_y'}{\sigma^2} = \frac{S(x'y')/N - A^2}{\mu_2} = .198.$$

Or we may use a short formula for the difference method.[9]

$$r = 1 - \tfrac{1}{2}\frac{\sigma_y^2}{\sigma_x^2} = 1 - .8024 = .198.^{10}$$

Where σ_y is the standard deviation of the difference between pairs.

Where the number of individuals in an array is very small the method presents no very marked advantages, but when the arrays are large it may be very useful and its range of applicability very wide.

For instance, one of the tests of the genotype theory of inheritance is to compare the correlation between parents and offspring with that between the parents co-fraternity and the offspring in a population of self fertilizing or vegetatively

[8] AMER. NAT., Vol. 44, pp. 693–699, 1910.

[9] Harris, J. Arthur, "A Short Method of Calculating the Coefficient of Correlation in the Case of Integral Variates," *Biometrika*, Vol. 7, pp. 215–218, 1909.

[10] On the basis of $N = 2239$, $r = .198 \pm .014$, and we may conclude that the individual plants are slightly but distinctly differentiated in their capacity for maturing seeds. In his English Series of Broom, Pearson (*Phil. Trans. Roy. Soc. Lond.*, A, Vol. 197, p. 335, 1901) found $r = .42$. The difference may easily be attributed to the smallness of the Woods Hole series, some, or possibly all, of the individuals of which may have come from the same parent plant. The case is an illustration of arithmetical method only.

propagating individuals.[11] The correlation surfaces are very easily prepared. Two seriation tables, one for the arrays from which the individual parents were drawn and one for the offspring arrays corresponding to each parental fraternity, are prepared. The first table is cut into strips by columns, passed strip by strip over the offspring seriation table, the frequencies which are in juxtaposition are multiplied together and summed simultaneously, and the resulting totals entered in the proper compartments[12] of a correlation table. This may be called an ascendant-descendant correlation surface. It includes both "parental" and "avuncular" relationships. The "avuncular" relationship is the one sought, and is quickly gotten by subtracting the surface for the relationship between individual parents and their offspring (which will have been already prepared for other purposes) from the ascendant correlation surface just described.

In a forthcoming paper I shall show how various correlations may sometimes be most easily determined from the first two moments for the individual classes or families without the labor of drawing up tables.

J. ARTHUR HARRIS.

COLD SPRING HARBOR, N. Y.,
July 7, 1911

ACQUIRED CHARACTERS DEFINED

IT is believed that if the term "acquired characters" is carefully defined, and the matter considered in view of that definition, a new light will be cast upon a generally misunderstood subject. The things to be defined are the verb *to acquire,* which means to obtain by effort, and the noun *character,* which means something forming part of an individual. The point of view here involved may be illustrated by the following quotation:

> "Some are born great,
> Some achieve greatness, and
> Some have greatness thrust upon them."

This shows three ways in which an individual obtains greatness. The same three ways apply to the different characters

[11] For an illustration see K. Pearson's analysis of Hanel's data for *Hydra grisea,* "Darwinism, Biometry and Some Recent Biology," *Biometrika,* Vol. 7, pp. 368–385, 1910.

[12] The compartments corresponding to the captions of the two columns dealt with.

which an individual has. Some characters are born in the individual, some are acquired, and some are thrust upon him.

A Roman nose or a pug nose is neither acquired nor thrust upon the individual. They are born to the individuals which possess them. The same is true of gray eyes or blue eyes, and many other characters. They are inborn and are matters of heredity.

A mutilation is not born to an individual or acquired by him. It is thrust upon him by accident or by the action of some other person, and against the will of the individual. A person is not born gray, nor does he achieve grayness. It is thrust upon him by age or by mental distress. We sometimes say that a person acquires a sun-burned face, but the statement is inaccurate. Sunburn is thrust upon the individual by the action of the sun's rays.

Those things which are *acquired* are obtained by the efforts, either physical or mental, of the individual which acquires them. But not all things which are acquired by efforts are "characters." Thus, a person may acquire wealth by his individual efforts, but wealth is not a character. It is a possession. An education is also acquired by efforts, but an education is a possession and not a character. So also athletes and race horses acquire records of various kinds, but these records are also possessions and not characters.

Characters are those things which pertain to and form a part of the individual as an individual. Acquired characters are not new characters, but characters which are changed from their normal inborn condition by their own functional activity, and are to be clearly distinguished from characters thrust upon a passive individual. Using a generic term to cover many specific qualities, it may be said that *strength* is the main if not the sole acquired character under this definition. Weakness is negative strength correlated with negative efforts.

Physical strength above the inherited strength is acquired by special physical efforts, such as training, and mental strength above the inherited mental strength is similarly acquired by special mental training. Conversely, physical strength is lost by the absence of physical efforts, and mental strength is lost by the absence of mental efforts. Efforts are something we can see and measure, and the relationship between these acquirements and the efforts by which they are obtained is such that we can measure the acquirements in terms of efforts.

It has been demonstrated beyond all question that those characters which are thrust upon the individual, such as mutilations, are not inherited. But how about those characters which are not thrust upon the individual but are acquired in accordance with the above definition? That there is a clear distinction between characters which are acquired and those which are thrust upon the individual is evident from the fact that neither mental nor physical strength can be thrust upon the individual. Irritation thrust upon an individual may in fact produce a great display of mental and physical strength, but the circumstance or the person who produced the irritation does not augment the strength which previously existed.

The above analysis shows that the individual after birth does or may come into possession of two kinds of characters, one of which is obtained by acquirement and the other of which is thrust upon him. Also, that those characters which are acquired can not be thrust upon him, and those which are thrust upon him can not be acquired. The distinction between the two kinds of characters is clear cut and marked. Evidence that one of these kinds is not inherited is not even presumptive evidence that the other is not. What is demanded is some clear and precise evidence in regard to those characters which are acquired and can by no possibility be thrust upon the individual.

R. L. REDFIELD.

NOTES AND LITERATURE

THE PRESENT-DAY CONCEPTION AND STUDY OF ANIMAL PSYCHOLOGY

THE latest issue—or one among the latest issues—in that long series of admirable books called the "Bibliothèque de Philosophie Contemporaine" (Felix Alcan, Paris) is Professor Georges[1] Bohn's "La Nouvelle Psychologie Animale." In 1909 Bohn published his interesting "La Naissance de l'Intelligence,"[2] in which he set out with characteristic French lucidity his position in the field of the new animal psychology, a position based at once on a considerable amount of personal observation and experimentation and on a thorough familiarity with the work of the whole modern school of experimental animal psychologists.

In the present smaller and simpler or more general statement —by general I do not mean vague—Professor Bohn restates with admirable understanding, restraint, definiteness and clearness, the conditions and the conclusions of the modern experimental study of animal reactions of the psychic and pre-psychic order. The conclusions, to be sure, can be held to be those of no other student than Bohn himself until other students give their formal adherence to them. But despite the inevitable disagreements about details, and the reluctance to subscribe to any man's dicta in toto, Professor Bohn's conclusions and attitude will find the adherence of a considerable body of animal psychologists.

The essentials of this position are its strongly "mechanical" tendency, *i. e.*, its attempt to make physics and chemistry and mechanics go as far as possible in explaining both stimuli and reactions; its strong leaning away from finalistic explanations, *i. e.*, the explanations of adaptation and of Darwinian selection; its rather sharp classification into three separate categories of the behavior of the invertebrates less the articulates, the articulates, and the vertebrates, respectively; its conception of these three categories of behavior as standing not as three successive stages in one grand line of evolution, but as of marking three fairly disparate lines of development, that is, of differing not so much

[1] Bohn, Georges, "La Nouvelle Psychologie Animale," pp. 200, 1911, Felix Alcan, Paris, 2 fr. 50 c.

[2] See review by the present writer in *Science*, November 5, 1909.

quantitatively as qualitatively; and, finally, its sharp and clear conceptions of what is meant by tropisms, by differential sensibility, by association of sensations and associative memory.

I have referred to the "strongly mechanical tendency" manifest in Bohn's attitude toward behavior explanations. This is not to align Professor Bohn with those representatives of the extreme mechanical school, such as Beer, Bethe and Uexkull. Recognizing the great service of these men in showing "the errors and the exaggerations" of the anthropomorphic or anthropocentric school of animal psychology, he expressly parts company with them when they push their way to the rigorously logical—or is it illogical?—extreme. Bohn says:

> With the vertebrates the psychic activity acquires, thanks to the brain, a very great complexity. It becomes useless to speak, in their case, of tropisms and of the elementary forms of differential sensibility, for one can no longer isolate these elements from the total activity. The memory has reached a considerable development, and, on the other hand, the chemism has lost its primitive simplicity; each remembrance, each idea can provoke new forms of sensibility.

Even with the articulates the "rudiments of associative memory are developed to the point of masking more or less the tropisms and the differential sensibility." Thus by the side of survivals of the older or simpler activities, the articulates reveal new psychic acquisitions due to associative memory.

For the behavior of the lower invertebrates, however, Bohn's explanations hold closely to those of the true mechanical school, those who rely on tropisms and differential sensibility. Bohn is highly appreciative of Loeb's pioneer work, the work that indeed marked the opening of the present epoch in animal psychology study. The originality and genuinely *Bahn-brechend* character of Loeb's work is freely recognized. Indeed, the whole work and activity of the present American school of animal psychology is referred to and praised over and over again in all of Professor Bohn's writings.

"La Nouvelle Psychologie Animale" has been "crowned" by the Académie des Sciences Morales et Politiques. It may also well be crowned by that reading public which likes its science authoritative and yet simple and clear and interesting at the same time.

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A USEFUL EVOLUTION BIBLIOGRAPHY

THE *Bulletin Scientifique de la France et de Belgique* (founded by Giard and now conducted by Blaringhem, Bohn, Caullery, Mesnil, Perez and others) has begun the issuance of a "Bibliographia Evolutionis" that promises to be of very great usefulness. Now that *L'Année Biologique* is irrecoverably out of date in its annual issues—has it indeed officially expired?—the new "Bibliographia Evolutionis" will be especially welcome.

It is published as a specially paged part of each issue (quarterly) of the *Bulletin,* so that at the year's end it can be gathered and bound up together either at beginning or end of the *Bulletin* volume, or, and better, separately.

The "Bibliographia" was begun in 1910 and in its first year (January–December, 1910) comprised careful abstracts and evaluations of 345 books and papers treating of such evolution subjects (the abstracts are arranged by subjects) as experimental biology, general cytology and fecundation, general embryology, phylogeny, evolution of instincts, general ethology, mimicry, symbiosis, parasitism, heredity and hybridization, influence of the environment and adaptations, regeneration, sex, castration and parthenogenesis, variation, teratology, general evolution. The books and papers reviewed are almost exclusively issues of 1910, only a few 1908 and 1909, references being included.

The "Bibliographia Evolutionis" apparently may not be subscribed for or purchased separately. To obtain it, then, one must subscribe for the *Bulletin Scientifique de la France et de la Belgique,* 32 francs a year, Redaction du Bulletin Scientifique, 3, rue d'Ulm, Paris. The *Bulletin* is, however, an admirably conducted journal, containing important original papers, by French and Belgian scholars, on evolutionary matters. It is at present in its forty-fifth volume (fifth volume of its sixth series).

Interesting item: the title page of the "Bibliographia Evolutionis" bears on its title page, as stamp of its recognition, a likeness of the face of Jacques Loeb.

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